Experiments on Choice under Uncertainty

Amrei Marie Lahno

Inaugural-Dissertation
zur Erlangung des Grades Doctor oeconomiae publicae
(Dr. oec. publ.)
an der Ludwig-Maximilians-Universität München

2014

vorgelegt von
Amrei Marie Lahno

Referent: Prof. Dr. Martin G. Kocher
Korreferent: Prof. Dr. Florian Englmaier
Promotionsabschlussberatung: 5. November 2014

Datum der mündlichen Prüfung: 22. Oktober 2014

Namen der Berichterstatter: Martin G. Kocher, Florian Englmaier, Andreas Richter

Danksagung

An dieser Stelle möchte ich all denjenigen herzlich danken, die zum Gelingen dieser Arbeit beigetragen haben.

Mein allererster Dank gilt meinem Doktorvater Martin Kocher, der mir die Möglichkeit gab, meine Promotion über die Verhaltensökonomie zu schreiben, welche mir vor einigen Jahren noch gänzlich unbekannt war. Ich bedanke mich für sein uneingeschränktes Vertrauen, seine Unterstützung bei all meinen Vorhaben und für die Freiheiten, die er mir während meiner Promotion gelassen hat. Mein Dank gilt auch meinem Zweitbetreuer Florian Englmaier, für seine persönliche Unterstützung und fachliche Hilfsbereitschaft hinsichtlich all meiner Forschungsprojekte, und meinem Drittgutachter Andreas Richter, der mich stets so wohlwollend in seine Forschungsgruppe eingebunden hat.

Ich danke meiner Kollegin Marta Serra-Garcia, mit der ich die ersten Kapitel dieser Arbeit mit viel Herzblut und Freude geschrieben habe. Ich danke Marta auch für ihre Unterstützung im Rahmen meines Forschungsaufenthalts an der University of California San Diego, sowie Uri Gneezy für seine Gastfreundschaft. Meinen Koautoren Stefan Trautmann, Ben D'Exelle und Arjan Verschoor gilt mein Dank für die vertrauensvolle Zusammenarbeit und für die zahlreichen fachlichen Anregungen während der letzten Jahre.
An dieser Stelle darf ich auch meinen Vater, Bernd Lahno, nennen, mit dem ich das einmalige Glück hatte, ein Familienprojekt in meine Dissertation einfließen zu lassen. Die Geduld und Unterstützung meiner Mutter darf hier nicht unerwähnt bleiben.

Ich danke auch meinen Kollegen an der LMU und an der Munich Graduate School of Economics für ihre Unterstützung, insbesondere Sebastian Strasser und Johannes Maier, sowie Dirk Reinhardt von der Munich Re Foundation, für das gemeinsame Engagement zum Thema Microinsurance.

Zu guter Letzt gilt mein großer Dank auch meinen Freunden, die mich während all der letzten Jahre in München begleitet haben, und natürlich meinen Eltern und Nils, die mich so häufig auf meinem Weg ermutigt haben und mir stets uneingeschränkt beiseite standen.

Amrei M. Lahno, Oktober 2014

Contents

List of Figures

List of Tables

Preface

Most everyday economic decisions involve uncertainty. Returns from investing in new technologies, such as machines or seeds, as part of small businesses, strongly depend on exogenous stochastic variables, such as weather conditions or non-verifiable qualities. Other practical examples include purchasing insurance, buying stocks, and investing in other financial products, which can be modeled as bets between the customer and the insurer, company or financial institution, respectively.

In economics, Expected Utility Theory is still widely used to explain choices under uncertainty, as formally specified by John von Neumann and Oscar Morgenstern in their 1947 book on game theory. Although theoretically appealing from a normative perspective, experimental economists identified numerous behavioral irregularities which cannot be accommodated in the expected utility framework. The common consequence and common ratio effects (also known as Allais' Paradox; Allais, 1953), the certainty effect (Kahneman and Tversky, 1986), loss aversion (Kahneman and Tversky, 1984; Tversky and Kahneman, 1991), and preference reversals (Tversky et al., 1990) are among the most prominent examples for such violations. Theoretical advancements, foremost (cumulative) Prospect Theory, proposed by Daniel Kahneman and Amos Tversky (1979, 1992), followed by Rank-dependent Expected Utility Theory (Quiggin, 1982, 1993), offer a descriptive formalization of such behavioral biases as rational choice.

In order to understand individual preferences in uncertain settings a distinction between risk and ambiguity is particularly necessary, as postulated by Frank Knight (1921) and John M. Keynes (1921). While risky prospects involve probabilities and outcomes which are exactly known, ambiguous prospects refer to unknown probability distributions. Initiated by the seminal experiments by Daniel Ellsberg (1961), many studies documented that people often prefer risky to ambiguous events, but might also be ambiguity seeking under other circumstances. Such behavioral biases are relevant in many practical applications, where individuals trade risk against ambiguity: investors might compare stocks to fixed-income products, or domestic to foreign assets; individuals might substitute insurance by (ambiguous) self-protection measures; and patients might waver between well-proven and new, but not fully approved, medical treatments.

Another problem of traditional accounts of decision-making under uncertainty is the common individualistic framework in economic theory. Yet, many empirical studies highlighted the role of others in individual choice. Suitably, already in the early 80's, Robert J. Shiller wrote that "investing in speculative assets is a social activity" (Shiller, 1984, p. 457).

Peer effects reflect differences in behavior of the very same individual observed in isolation from any others, compared to a social setting in which interaction with others is possible. Peer effects have received great attention in the psychological and economic literature, and are prevalent in various environments, ranging from effort provision in the workplace (e.g., Falk and Ichino, 2006) and pro-social behavior (e.g., Gächter et al., 2013), to performance in education (e.g., Sacerdote, 2001). In this dissertation I particularly focus on peer effects in decision-making under uncertainty, where one may think about how feedback about others might influence one's own investment strategies, take-up of loans, or purchases of insurance policies.

There are multiple reasons why peers may influence individual choices, inter alia three key rationales which refer to distributional concerns, to a taste to conform to others, and to information. The most important attempts to understand the influence of others on individual choice in economics are probably based on the concepts of outcome-based social preferences, as introduced in the seminal contributions by Ernst Fehr and Klaus Schmidt (1999) and Gary Bolton and Axel Ockenfels (2000). Such models may incorporate non-selfish individuals who dislike inequitable outcomes, and thereby accommodate feelings caused by social comparison, such as envy or jealousy. Yet, other-regarding preferences do not necessarily depend on comparison of outcomes. Conformism, as established in social psychology (see e.g., Festinger, 1954; Cialdini and Goldstein, 2004), describes that individuals generally wish to conform to "social" decision anchors, independent of outcome comparisons. The experiment of Solomon Asch (1956) is just one well-known example that demonstrates how easily individuals might follow the crowd. Finally, theories of social learning assume that choices of others might convey information, which may particularly matter in settings of asymmetric information. In developing countries, many people just gain access to financial markets, may be inexperienced with finance, or even illiterate. But even in developed economies, the diversity of financial products in (developed) financial markets portrays an increased complexity of economic decisions. Basically, learning from others might seem advantageous when decision-making appears difficult.

Understanding whether these three rationales actually drive peer effects in risk taking is of utmost importance in many respects. A spread of investments in high-risk products in societies initiated, e.g., through word-of-mouth recommendations or comparisons with others, might be undesirable from a policy perspective. The large trading volume of financial assets, such as derivatives which became a common investment even in traditional banking, plainly demonstrates potential adverse effects on individuals' safety nets. On the contrary, policy makers may also be interested in the spread of particular behavior, and they might be able to employ social measures, for example, to leverage the demand for microinsurance in developing countries.

This dissertation consists of five essays on individual decision-making under risk and ambiguity, and social interaction. All of them are based on laboratory experiments which are nowadays well established in economic research, since they allow to draw causal inferences from exogenous treatment variations to behavior. In the context of this dissertation, experiments provide the central tool to elicit context-free individual preferences, and to identify peer effects through clean variations in the decision-making environment.
Chapters 1 and 2 deal with decision-making under risk. We examine different sources of peer effects in risk taking in the first chapter, and study how differences in attitudes towards risk might relate to social relationships in the second chapter. Chapters 3 and 4 focus on decision-making under ambiguity. In the third chapter, we picture an anatomy of ambiguity attitudes in accordance to different outcome domains and probability distributions. How peer effects might influence ambiguity attitudes is examined in the fourth chapter. Chapter 5 provides a test of the theory of Team Reasoning. This chapter goes beyond individual decision-making, but parallels the previous chapters in that we focus on decision-making in teams, where social interaction is inevitable.

In the first chapter, which is joint work with Marta Serra-Garcia, we study peer effects in risk taking. In particular, we test whether peer effects can be explained by preferences over others' payoffs, preferences over others' choices, or both. We thereby contribute to the theoretical and experimental literature which has devoted most attention to distributional social preferences as an explanation for imitative behavior.
We design an experiment in which subjects make a series of risky choices between simple binary lotteries. Eliciting choices individually and again in groups of two subjects, a decision maker and his peer, allows us to cleanly identify peer effects which we define as strategies by which a decision maker chooses *not* to stay with his individual choice. Across three treatments we either allow the peer to choose among lotteries, we randomly allocate her a lottery, or we ask her to make a random draw which is completely unrelated to the lotteries or payoffs.[1] We then elicit the decision maker's choice conditional on the peer's choice, allocation, or random outcome. In order to asses *how* peer effects shape individual behavior, additionally to a quantitative assessment, we use the strategy method. This allows us to distinguish between four feasible (pure) strategies: to imitate and to deviate from the peer (conditional strategies), and to change and to keep the individual choice, both irrespective of the peer's choice, allocation or random outcome (unconditional strategies).

[1] In all chapters of this dissertation I refer to a decision maker as "he". In this chapter, we additionally refer to the peer as "she".

Our results reveal two essential findings. First, somewhat surprisingly, the possibility to condition on a peer's allocated lottery does not lead to stronger peer effects than the possibility to condition on an unrelated outcome of the peer. This may reflect indifferent preferences to some extent, but it also suggests that the mere possibility of *conditional* choices already induces changes in individual behavior. At the same time, decision makers are equally likely to play an imitation and deviation strategy with respect to the unrelated outcome (where imitation and deviation is defined rather arbitrarily), but they are significantly more likely to imitate the lottery allocated to the peer. This strongly indicates that – conditional on being affected by the peer's presence – decision makers exhibit relative payoff concerns which are such that imitation is optimal. Second, peer effects increase significantly when the peer chooses instead of being randomly allocated a lottery, and the frequency of imitation almost doubles. This clearly demonstrates that choices of peers matter, above and beyond their direct impact on payoffs.

Why is this the case? We elaborate on two possibilities. First, relative payoff concerns might change if peers actively choose. Intention-based models of social preferences, for example, suggest that a feeling of envy may increase if peers are accountable for their decisions. And fairness considerations may reinforce this feeling when peers choose a relatively safe option.[2] Alternatively, following the ideas of social comparison theory (Festinger, 1954), choices of peers might be perceived as a decision anchor and measure for "correctness", thereby inducing a norm to conform to others' behavior. Both models allow us to derive testable predictions on comparative statics and can be structurally estimated. Overall, we find that our data is at odds with a flexible specification of relative payoff concerns, but consistent with a norm of conformism.

Our study is one of the first to distinguish between outcome-based and choice-based social preferences as explanations for peer effects in risk taking. Our main contribution is to show that peer effects in risk taking cannot only be explained by relative payoff concerns, and, hence, to demonstrate that a parsimonious explanation of preference interactions in risk taking needs to allow active or passive choices to matter. Beyond research, our results also have important implications for the spread of risky behavior in a society. They suggest that communicating others' risky choices may have large consequences even if everyone is equally well informed. At the same time, the perception of accountability – for example, whether peers chose their pension plans on their own responsibility or simply benefit from company policies – might crucially influence imitative behavior and the effectiveness of providing social decision anchors. Campaigns that endow individuals with financial products to leverage demand, such as obligatory insurance which is bundled with take-up of loans in developing countries, may only have limited success.

[2]Intention-based models of social preferences were proposed, for example, by Blount (1995) or Bolton et al. (2005). Fairness consideration in risk taking were examined by, e.g., Cappelen et al. (2013).

The second chapter, which is joint work with Marta Serra-Garcia, Ben D'Exelle, and Arjan Verschoor, also contributes to the literature on risk taking, by linking risk attitudes to social relationships. More precisely, we examine whether the pervasiveness of interpersonal conflict between two individuals is related to differences in their attitudes towards risk. This study builds on fieldwork in more than thirty villages of rural Uganda, in which we conducted a survey to identify social relationships on a village level and to gather socio-economic information, followed by an experiment to elicit risk attitudes. We focus on a society, the Bagisu people, which historically suffered from violent conflicts among each other, as well as severe hierarchical and gender ideals.[3] Of course, interrogating about conflictual disputes among village members required a particularly sensitive design of questionnaires.

Linking individual attitudes towards risk to conflictual disputes seems straightforward in many settings, especially in developing countries. In these regions, investments in the context of farming, for example, are often made jointly by groups, and investments yield only uncertain returns, given unsteady weather conditions and risks inherited in new technologies. Differences in the willingness to take risks might hinder agreements in bargaining over investments into new machines, seeds, or other technologies. Other examples include informal risk-sharing arrangements (IRSAs) in case the investment goes wrong; and gifts or informal loans to help finance an investment, often with an expectation of reciprocity.

Our analysis reveals a persistent and significant relationship between the presence of conflict and differences in risk attitudes, controlling for other relevant individual and pair characteristics. Interestingly, this relationship is particularly strong among kin and between males. But since the composition of rural villages, with respect to distributions of risk attitudes and the presence of conflicts, cannot be exogenously changed, our results cannot be interpreted as causal evidence. Yet, using a simulation approach, we argue that the relationship seems to be of causal nature, in the sense that differences in risk attitudes directly increase the likelihood for interpersonal conflict, instead of vice versa. Given that we only documented social links between participants from the same village, out of which only one pair reports not to be acquainted, we extrapolate our analysis to links between individuals who are very unlikely to know each other by randomly generating links across villages. If conflicts are likely to severe social relationships, as a result of which risk attitudes might diverge, then differences in risk attitudes should be similar across random and conflict links. However, we show that this is not the case. Our results are also robust against selection effects, given that we do not find that risk attitudes are per se correlated to conflict or personal attributes which are related to the exposure of conflict.

To our knowledge, we are the first to study the determinants of interpersonal conflict in

[3] Suzette Heald (1998) provides an excellent account about the Ugandan society and the Bagisu people.

the microeconomics literature which rather focuses on friendships or generally positive social ties, relative to links with only little or no depth. In that sense, we provide novel evidence on how fragile interpersonal relationships might be under heterogeneity in risk attitudes, particularly among individuals who are likely to be involved in joint economic decisions. Our results may help us understand future conflict between groups, including small societies, kin or teams in organizations. Especially in developing countries, people in small-scale societies are tied through informal financial arrangements in myriad ways, which may imply a tremendous scope for disagreement, and if not settled, for conflict.

The third chapter is joint work with Martin Kocher and Stefan Trautmann and establishes the step from decision-making under risk to decision-making under ambiguity. This work is motivated by an overwhelming number of experimental papers that document varying ambiguity attitudes in different settings, and theoretical papers that mostly assume a generally negative attitude towards ambiguity. Daniel Ellsberg (1961) was the first, followed by many other experimentalists, to cleanly document that individuals tend to exhibit ambiguity aversion if presented with lotteries in the domain of moderate likelihood gain events. Theoretically, ambiguity aversion was proposed to explain the equity premium puzzle, a home bias of investors, or low take-up rates of genetic tests.[4] However, for unlikely events and losses, experimentalists also found ambiguity seeking, and report rather limited evidence for mixed domain events. In terms of applicability, as with regard to stock market performance or consequences of testing certain medical treatments, outcomes likely refer to gain and loss events, and (objective and subjective) probabilities are not necessarily of moderate size.

Yet, experimental papers mainly measured ambiguity attitudes in very particular settings, i.e., only a few documented attitudes for gains *and* losses, as well as for small *and* moderate probabilities at the same time. In particular, the fact that elicitations methods vary considerably across studies, inhibits to picture a clean anatomy of ambiguity attitudes. This is exactly what we aim for. We contribute to the literature on decision-making under ambiguity by collecting a rich dataset on individual attitudes, in the most common combinations of outcome and likelihood distributions. In our experiment, we elicit ambiguity attitudes of roughly 500 subjects across seven treatments, for prospects over gains, losses, or mixed outcomes, involving small or moderate likelihoods, respectively.

Unambiguously, our findings confirm the conjecture that ambiguity aversion is by far not the predominant attitude. At a first glance, we replicate the persistency of ambiguity aversion in the standard Ellsberg setting, while we also find evidence for ambiguity seeking, particularly for small likelihoods. Digging a bit deeper into preferences, we find that a substantial fraction of subjects in fact exhibits preferences close to neutrality. But abstracting from those lets the typical fourfold pattern of ambiguity attitudes unfold: we

[4] See Collard et al. (2011); Epstein and Miao (2003); Hoy et al. (2014), for an example, respectively.

observe a reflection effect between gains and losses, with ambiguity aversion (seeking) for moderate gain (loss) events as well as for low likelihood loss (gain) events. Results on mixed domains further suggest the existence of ambiguity seeking.
It was in fact Daniel Ellsberg (2011) himself, on the occasion of his article's 50th anniversary, who argued that the fear of a bad unknown probability might be an artifact of the particular experimental setting. Consistently, our results strongly suggest that ambiguity seeking will empirically be equally relevant as ambiguity aversion, especially in situations in which people might hope for better odds by choosing the ambiguous alternative.

In the fourth chapter I examine ambiguity attitudes and whether feedback about others' choices provides an anchor for individual decisions, thereby linking the first and the third chapter. In contrast to the first chapter in which we examined specific social preferences behind peer effects in risk taking, I here particularly focus on whether peer effects may depend on characteristics of the ambiguous setting.
Numerous field studies documented peer effects in economic decisions characterized by ambiguity. But although identifying dynamics in ambiguity attitudes and their (social) determinants is hard in the field, laboratory studies mainly studied determinants of peer effects in risky settings. Yet, given a body of theory which explains suboptimal economic decisions by ambiguity aversion, e.g., in finance or health, understanding how such attitudes and biases in probabilistic sophistication might be affected by others, has important implications.
Following the experimental design from the third chapter, ambiguity attitudes are elicited in the standard Ellsberg setting with moderate likelihoods, individually in a first part, and again in a second part. Between subjects, I vary whether participants learn previous choices of a peer when making their choices a second time (which I label a *social anchor*), and whether prospects are defined over gains or losses. Building on documented patterns of ambiguity attitudes with respect to outcome domains (as supported in the previous chapter), this design allows me to study how peer effects differ according to initial attitudes and outcome frames.
My analysis reveals one key finding, that is, individual dynamics and peer effects in ambiguity attitudes considerably differ between the domains of gains and losses. In the domain of gains, learning to be more ambiguity averse than a peer significantly increases the likelihood to change, relative to having no social anchor available. Also, decision makers tend to imitate their peer's attitude, towards ambiguity aversion, seeking, or neutrality. In the domain of losses, in contrast, learning to exhibit exactly the same attitude as a peer significantly reduces the likelihood to change, relative to an individual condition. In this case, I predominantly observe shifts towards neutrality, however, such movements even exists if a social anchor is not available. This suggests that ambiguity seeking might not be particularly robust in the long run. But generally, the *relative*

ambiguity attitude, i.e., the ambiguity attitude compared to the peer's, matters, although differently depending on the outcome domain. Further, the provision of a social anchor ultimately induces ambiguity neutrality on the aggregate level.
Models on rational learning might suggest that Bayesian decision makers converge towards ambiguity neutral preferences, and two experimental studies by Keck et al. (2011) and Charness et al. (2013), who examine changes in ambiguity attitudes after face-to-face consultation with others, validate that social interaction might induce shifts towards ambiguity neutrality. In contrast, social preferences models on conformism or distributional concerns might generally predict imitative shifts towards the peer's attitude. My findings provide evidence for both lines of argument, but they particularly corroborate the conjecture of Keck et al. (2011) and Charness et al. (2013), namely that ambiguity neutrality may be established as a persuasive argument, at least in the domain of gains.

A project that goes beyond individual decision-making by considering a model of collective agency, constitutes the fifth chapter of this dissertation, and is joint work with Bernd Lahno. In this chapter we discuss and experimentally test the theory of Team Reasoning (TR) as a guide to coordination in common dilemma problems of strategic interaction. TR was proposed by Robert Sugden (1993, 2000, 2003) and Michael Bacharach (1999, 2006) as an attempt to overcome shortcomings of traditional Rational Choice Theory, in particular its inability to explain equilibrium selection in simple coordination games. Often, our common senses suggest an evident strategy to reach coordination, which makes it even more frustrating that Rational Choice Theory – the central theoretical model of strategic interaction – neither offers an appropriate solution concept, nor provides a normative justification of the obvious "rational" route to coordination.
We analyze how TR implements team agency into the theory of rational action, in order to formalize the idea that individuals often decide from a team perspective, which may automatically unveil common routes to coordination. Intuitively, everyone should do his part to achieve the best possible outcome for the group. We elaborate on the fundamental assumptions adopted from instrumental rationality, showing that TR still inherits the idea of opportunistic choice, including one important detail: in theory, a team can derive a decision only on information commonly available, hence, its members should not use their entire private information. In an experiment in which teams are given a chance to coordinate on a particular pattern of behavior before a modification of the stage game offers opportunities to improve on the team goal, we test whether such restrictions are consistent with actual behavior. Our observations throw considerable doubt on the idea of opportunistic team reasoning as a guide to coordination. First, we find that individuals tend to stick to accustomed behavioral patterns, while TR categorically denies such influence of past behavior on future choices. Second, we find significant differences in the behavior of subjects in accordance with their particular situation, suggesting that indi-

vidual behavior is at least partly determined by private information, in contradiction to optimal play from a team perspective.

Why does TR obviously fail, albeit its intuitive plausibility, and although it seems so appealing as a normative theory of choice? Our results may indicate a misapprehension of the triggers for reasoning as a team, rather than a misconception of TR as such. According to Bacharach, acting as a "teamer" requires team identification; Sugden additionally adds that "teamers" need mutual assurance that their peers reason in the same way. We argue that both conditions are not sufficiently exclusive, and too easily fulfilled. Our discussion also highlights that TR does not ascribe the reasoning process itself to collectives, but to each individual's mind. Without doubt, modeling collectives agency is hard theoretically. But teams might act in their own momentum: peers communicate and collaborate, such that the team itself would constitute the agent who actually makes a decision.

All five chapters are self-contained in the sense that they include their own introductions and can be read independently. The respective appendices are attached in chronological order to the end of the fifth chapter, and contain the instructions of the experimental protocols as well as supplementary figures and tables.

Chapter 1

Peer Effects in Risk Taking*

1.1 Introduction

Decision-making under risk is mainly studied at the individual level. Yet, an increasing body of research documents peer effects in risk taking. Peers have a large impact on stock market participation (e.g., Shiller, 1984; Hong et al., 2004), investment decisions (Bursztyn et al., 2014) and insurance choices (Cai et al., forthcoming), among others.[1] A main source of peer effects are preference interactions, in the terminology proposed by Manski (2000), whereby individual preferences depend on the actions of others.[2] A key open question is, *how* do preferences depend on others? Which factors matter?
As Manski (2000) writes, preference interactions may arise from "everyday ideas" such as envy or conformism. In other words, in environments with complete information, peer effects may be generated because individuals care about other's outcomes (envy) or because they care about other's choices (conformism), or both. Much attention in the literature on peer effects has been given to envy, a central concept in models of distributional social preferences.[3] These types of preferences have been used to explain peer effects in risk taking, for example, in asset pricing (e.g., Galí, 1994; Gebhardt, 2004, 2011). Less attention has been given to preferences where the choices of peers, irrespective of their externalities on outcomes, have a direct impact on an individual's behavior. A central rationale why choices may matter is provided by studies in social psychology, which show

*This chapter is based on joint work with Marta Serra-Garcia.

[1]Peers might generally influence risk and other economic attitudes (Ahern et al., forthcoming). Peers also affect credit decisions (e.g., Banerjee et al., 2013; Georgarakos et al., 2013), savings decisions (e.g., Duflo and Saez, 2002; Kast et al., 2012) as well as different teenager (risky) behaviors (for an overview, see Sacerdote, 2011). Generally, peer effects are important in education (e.g., Sacerdote, 2001; Duflo et al., 2011), in labor (e.g., Falk and Ichino, 2006; Mas and Moretti, 2009; Card et al., 2010), and pro-social behavior (e.g., Gächter et al., 2013).

[2]See Manski (2000) for an overview of the sources of social interaction effects, which include market interactions, expectations interactions and preference interactions.

[3]Different models of distributional preferences have been proposed in the literature, for example, by Fehr and Schmidt (1999), and Bolton and Ockenfels (2000) (for a survey, see Camerer, 2003; Fehr and Schmidt, 2006).

that individuals are often driven by a norm to conform to others' behavior (e.g., Cialdini and Trost, 1998; Cialdini and Goldstein, 2004). In models of conformity, others' choices provide a social anchor to which individuals conform (Festinger, 1954). However, most existing studies on risk taking focus on the effect of outcome comparisons: in the typical settings peers either do not choose, or their payoffs and choices are directly linked. Hence, one cannot distinguish between explanations for peer effects based solely on distributional social preferences and those that allow choices to matter. This paper examines peer effects in simple decisions under risk and tests whether these can be explained by preferences over others' payoffs, preferences over others' choices, or both. The main contribution is to show that peer choices play a significant role and, hence, that peer effects (in our environment) can only be explained by a combination of preferences over others' payoffs *and* choices.

To be able to cleanly identify peer effects, we use a controlled lab experiment in which individuals make risky choices, first individually and then in groups of two.[4] One player is assigned to be the first mover (peer) and the other the second mover (decision maker). Risky choices are made between two simple lotteries, with at most two outcomes and same probabilities, and there is complete information.[5] We examine behavior in three main treatments. In the first treatment, the peer chooses among lotteries (Choice treatment). In the second treatment, the peer is randomly allocated a lottery (Rand treatment). In the third treatment, as a control, the peer is asked to make a random draw, an act completely unrelated to lotteries or payoffs. Since there are only two equally-likely outcomes, we refer to this as the Coin treatment.

A further question we address is *how* decision-making changes in the presence of peers. Depending on the type of preference interaction, peer effects may lead to imitation or deviation (Clark and Oswald, 1998). To identify the direction of peer effects, as well as to avoid feedback effects, we elicit the decision maker's choices conditional on the peer's choice, allocation or unrelated act. This allows us to observe four different strategies. The decision maker may condition his choice on the peer's choice, allocation or act, by either imitating or deviating.[6] Or the decision maker may choose not to condition. In this case he either makes the same or changes the choice that he made individually, both being irrespective of the peer's choice, allocation or act. We say peer effects occur if the decision maker chooses *not* to stay with his individual choice.[7]

[4] See Manski (1993) for a discussion of the problems with the identification of peer (or social interaction) effects.

[5] We label one lottery as riskier than the other, in terms of variance.

[6] In Coin, the definition of imitation and deviation is arbitrary, as the decision maker cannot condition on the lottery of the peer but an unrelated random outcome, odd or even, as we detail below.

[7] We define peer effects with special respect to our experimental setting. We may, for example, also define peer effects in terms of imitation and deviation, in which case our main results would still hold. However, since decision makers are confronted with a peer in every treatment, we use the most careful definition of peer effects.

Our results show that peer effects differ significantly when the peer is allocated a lottery compared to when he chooses a lottery, though payoffs remain constant across treatments. Decision makers choose not to stay with their individual choices in 18% of the cases in Rand, and in 33% of the cases in Choice, i.e., peer effects almost double. Hence, choices of the peer matter, above and beyond their direct impact on payoffs. Second, peer effects are not significantly different between Rand and Coin, where decision makers change their individual choices in 17% of the cases. Thus, somewhat surprisingly, the possibility to condition on a peer's allocated lottery does not lead to stronger peer effects than the possibility to condition on an unrelated act of the peer.

In Coin and Rand, decision makers exhibit a similar likelihood to condition their choices on the peer. However, while they are equally likely to imitate or deviate in Coin, they are significantly more likely to imitate in Rand. This indicates that, conditional on being affected by the peer's presence, individuals seem to exhibit preferences over others' payoffs and, in particular, these are such that individuals prefer to imitate their peers. From Rand to Choice, the increase in peer effects mainly stems from a significant increase in the frequency of imitation. Hence, in our environment, peer effects cannot *only* be explained by concerns about others' payoffs relative to own. This implies that a parsimonious explanation of preference interactions in risk taking needs to allow peer choices to matter.

We examine two alternative explanations for why peer choices matter. First, one may consider a more flexible specification of distributional preferences, by which these change depending on whether the peer makes choices or not. Intention-based models of social preferences (e.g., Blount, 1995; Bolton et al., 2005) and studies of fairness considerations in risk taking (e.g., Cappelen et al., 2013) suggest that the strength of relative payoff concerns might depend on whether the peer actually makes a choice and, if so, whether she chooses to take on more or less risk than the decision maker. We examine whether this can explain the increase in peer effects from Rand to Choice by deriving predictions on comparative statics across treatments under such assumptions. First, we consider an increase in envy, i.e., the disutility from falling behind the peer, when the peer makes choices. This would increase the likelihood of imitation and, at the same time, increase the importance of expected payoff differences between lotteries, as these are relevant for (expected) payoff comparisons. We also examine the possibility that, when peers make choices, individuals especially dislike falling behind a peer who makes a safe choice. In this case, when the peer chooses among lotteries, we should observe a stronger increase in imitation towards the safer compared to the riskier lottery.

A second, alternative explanation for the increase in peer effects is, broadly speaking, that individuals are influenced by a norm to conform to others. More specifically, according to Festinger's (1954) theory of social comparison, individuals care about making correct choices and, in the absence of objective measures of correctness, consider others' choices as an anchor for correctness. Hence, if individuals exhibit such a preference to conform, peer choices should matter. They should increase the likelihood of imitation in Choice and this increase should not systematically depend on the type of lottery, risky or safe, or its expected payoff.

Given there was complete information, peer effects in our experiment cannot be explained by a model of rational social learning (e.g., Bikhchandani et al., 1998).[8] In the presence of complete information, under standard assumptions of rationality and self-interest, decision makers do not learn from others.[9]

Our results reveal that the increase in imitation from Rand to Choice occurs both towards safer and riskier lotteries. At the same time, the expected payoffs of the lotteries do not play a systematic role in the increase in imitation. The latter suggests that the increase in imitation is not driven by an increase in envy. The former is at odds with an increase in envy that is dependent on how much risk the peer chooses to take. These findings are broadly in line with an explanation that choices matter due to a norm to conform to others. As an additional test, we structurally estimate a model of relative payoff concerns and a model based on social comparison theory, where individuals derive a constant utility from conforming to the peer's choice or allocation. We allow utility parameters to vary across treatments and find that, under a model of relative payoff concerns, preference parameters do not change significantly when moving from Rand to Choice. In contrast, the utility from conforming increases significantly in Choice. The model based on social comparison theory fits our data significantly better, which provides further suggestive evidence that a reason why choices of peers matter may be due to a norm to conform to others.

Recent laboratory experiments have documented peer effects when peers are allocated lotteries (see Trautmann and Vieider, 2011, for an overview). Bault et al. (2008), Rohde and Rohde (2011) and Linde and Sonnemans (2012) report that lotteries and outcomes allocated to peers affect, in varying degrees, individual risky choices and emotions. Our control treatment, Coin, provides new insights relative to these previous results. It reveals that unrelated acts of peers may generate equally large peer effects. Allocations of the peers' lotteries may hence be seen as affecting the type of peer effect, imitation versus deviation, conditional on there being a peer effect. Two other related studies show that

[8] To increase the salience of complete information in our experiment, instructions were read aloud, for both potential roles in the experiment, and roles were assigned randomly within the same session. Also, we designed the lotteries to have at most two outcomes to minimize complexity. For a given probability distribution with respect to the good and bad outcome, we implemented six pairs of lotteries, keeping the risky lottery fixed. The safe lottery was degenerate for half of choice situations, and involved two outcomes for the other half. The number of outcomes of the safe lottery, which can be viewed as a measure of complexity, does not have a significant influence on peer effects.

[9] However, decision makers who exhibit preferences for conformity may learn about the correctness of their choice. In the famous experiments on conformity by Asch (1956) individuals conform to peers choosing an incorrect answer, though individually they are able to identify the correct answer.

observing either the desired risky choices of others (Viscusi et al., 2011) or their past choices (Cooper and Rege, 2011) significantly affects risk taking. In Cooper and Rege (2011) individuals are put in groups of six and provided either with private or social feedback. The comparative statics reveal that individuals move to safer choices with social feedback. This suggests that social regret, a form of relative payoff concerns, is a better explanation for the peer effects in their setting compared to conformity. In line with their results, we find a similar movement towards safer choices when decision makers can condition on the peer's lottery, within each treatment, Rand and Choice. A main contribution of our study is to show that, over and above relative payoff concerns, the choices of peers play a significant role. Hence, when modeling preference interactions in risk taking it may be misguided to focus only on relative payoff concerns.

Our experiment also complements studies testing the channels of peer effects in other environments. Gächter et al. (2013) and Goeree and Yariv (2007) examine whether peer effects are driven by distributional social preferences or social norms (or a norm to act like others), in a gift-exchange game and a social learning environment, respectively. While Gächter et al. (2013) find that peer effects can be explained by distributional social preferences, Goeree and Yariv (2007) find that conforming behavior cannot be explained by distributional social preferences, but is consistent with a preference for conformity.[10] Additionally, two recent field experiments focus on separating social learning from preference interactions. Cai et al. (forthcoming) show that social learning matters most in the context of rainfall insurance in China, while Bursztyn et al. (2014) find that preference interactions also play a significant role in investment decisions in Brazil.

In different environments, policy makers as well as private companies may be interested in influencing risky choices of individuals. In this respect, our findings can be important from a policy perspective. Our results suggest that whether peers are viewed as having made choices, e.g., they have actively chosen to buy insurance or to purchase a financial product, relative to having been endowed or given that same product, may be important for the spread of risky choices. A specific example are pension plan choices, where peers may be viewed as having chosen a plan or as following the default plan (under automatic enrollment).

The remainder of the paper is organized as follows. In section 1.2 we describe the experimental design and procedures in detail and derive testable hypotheses. Our main results are presented and discussed in section 1.3. Section 1.4 concludes.

[10] There are a variety of studies examining social comparison effects in games such as public good games or coordination games (e.g., Falk and Fischbacher, 2002; Falk et al., 2013). In social learning environments, Çelen and Kariv (2004) also study herding behavior, and identify substantial herding behavior.

1.2 Experimental design

1.2.1 Treatments

Our experiment elicits multiple choices between two lotteries, A and B, with at most two possible outcomes. A always has a larger variance than B. We refer to A as the risky option or lottery, and B as the safe one.[11] The exact lotteries are described in section 1.2.2 below.

In the first part of the experiment (Part I), subjects make lottery choices individually. In the second part (Part II), they make the same choices, but in a different order, and in groups of two. In each group, one subject is assigned to be *first mover* and the other to be *second mover*.[12] We consider a weak form of a peer: The decision maker (second mover) only knows that the peer (first mover) is a subject in the same session, but she remains anonymous throughout.[13] In Part II risks are *perfectly correlated* across group members: a single draw of nature determines the payoffs of both members. Perfect correlation is common in risk taking environments in which peer effects have been studied. Among others, risks are perfectly correlated in stock purchases as well as for many investment products, such as that considered by Bursztyn et al. (2014). They are also almost perfectly correlated in the weather insurance considered by Cai et al. (forthcoming).[14]

In our two main treatments the decision maker can condition his choice in Part II on his peer. In the first treatment (Rand) the peer does not make a decision in Part II, instead she is exogenously (randomly, with equal probability) allocated lottery A or B. In the second treatment (Choice), the peer chooses lottery A or B. Additionally, in a control treatment (Coin), the decision maker can make choices conditional on the peer, but based on a dimension that is unrelated to the lottery choice situation. At the end of the experiment, the peer rolls a computer-simulated die, by clicking a button on the screen, and the decision maker can condition his choices on whether the outcome is *odd* or *even*. For simplicity, we refer to this as a coin flip.

We use the strategy method in Part II, which allows us to observe the strategy of the decision maker conditional on the two possible choices, allocations, or the unrelated act of the peer. This allows us to examine four potential strategies of second movers:

i) *Imitate*: choose A if the peer has A or *odd*, B if the peer has B or *even*,

[11] In terms of risk preferences B cannot be labeled as safe since it does not necessarily yield a certain payoff. In comparison to A, we still label it as safe, for simplicity, as its variance is always smaller. But note that a risk averse individual does not necessarily prefer B over A.

[12] Groups remain the same for the whole of Part II. All choices are made without any feedback until the end of the experiment. During Part I participants only know there will be a Part II in the experiment, but do not know anything about the decisions they will be asked to make.

[13] Throughout, we will refer to the peer as "she" and the decision maker as "he".

[14] At the end of the experiment, individuals are informed about their payoff and, if Part II is drawn for payment, the choice and payoff of the other individual in the group.

ii) *Deviate*: choose A if the peer has B or *even*, B if the peer has A or *odd*,

iii) *Revise* own choice: make a different choice than in Part I, independent of the peer,

iv) *No change*: make the same choice as in Part I.

Note that in Coin the definition of imitation and deviation is arbitrary, as there is no direct link between the lottery choice of the decision maker and that of the peer. Additionally, while the last strategy, no change, implies the absence of a peer effect, the first three strategies all involve different forms of peer effects. As an overall measure, we define a peer effect to occur if the individual *switches*, i.e., chooses a different lottery in Part II than in Part I for at least one potential choice, allocation or act of the peer (also compare footnote 7).

The strategy method avoids any feedback effects, by keeping information about the risk preferences or consistency of the peer absent, during the experiment. At the same time, it may potentially affect the choices made by subjects, which we can control for by analyzing conditional choices in the Coin treatment. Brandts and Charness (2000) actually find that the strategy method does not generally generate differences in treatment effects, and Cason and Mui (1998) do not find an effect of the strategy method in a dictator game where the effects of social information are studied.[15]

1.2.2 Lotteries

The lotteries presented to subjects are summarized in Table 1.1. A yields the same payoffs throughout, a payoff $m_A^g = 20$ in the good state (g) which occurs with probability p, and a payoff $m_A^b = 0$ in the bad state (b). The payoffs of B are similar to those of an insurance product, $m_B^g = 20 - (1-p)cf$ and $m_B^b = 0 + c - (1-p)cf$, with the same probabilities as A. Compared to A, in each state a "premium" of $\delta = (1-p)cf$ is subtracted, while in the b state B pays a coverage c. We vary c, p and f across decision problems.

We use the notation $m^g{}_p m^b$ in Table 1.1 to define a lottery that pays m^g with probability p and m^b with remaining probability $1-p$. First, we divide the lotteries into three groups: lotteries with $p = 0.2$, $p = 0.5$ and $p = 0.8$, and within each group we create six decision problems: two with $f = 1.2$, two with $f = 1$ and two with $f = 0.8$. Throughout the paper, when $f = 0.8$, B has a higher expected value than A ($EV_B > EV_A$), when $f = 1$, $EV_B = EV_A$, and when $f = 1.2$, $EV_B < EV_A$.[16] For each possible combination of p and

[15] Similarly, in two additional treatments, we do not find evidence suggesting the strategy method had an effect in our setting. More specifically, we conducted a Base treatment in which choices were made twice, in Part I and Part II, without the strategy method and without social feedback. We also conducted an Anticipation treatment, without the strategy method, but where individuals were aware they would be given feedback about the peer's choice *at the end* of the experiment. Consistent with the effects of our main treatments, we observe peer effects increase significantly with anticipated social feedback, from occurring in 6.7% of the decisions in Base to 17.5% in the Anticipation treatment (Mann-Whitney test, p-value=0.016).

[16] We will use the terms expected value and f interchangeably.

f, c is either 20 or 15. We label lotteries with $c = 20$ as *certainty lotteries*, and those with $c = 15$ as *uncertainty lotteries*.[17]

Nr.	Lottery A	Lottery B	c	f	EV_A	EV_B
		Panel A: 20/80 Lotteries				
1	$20_{0.2}0$	0.80_1	20	1.2	4.00	0.80
2	$20_{0.2}0$	$5.60_{0.2}0.60$	15	1.2	4.00	1.60
3	$20_{0.2}0$	4.00_1	20	1.0	4.00	4.00
4	$20_{0.2}0$	$8.00_{0.2}3.00$	15	1.0	4.00	4.00
5	$20_{0.2}0$	7.2_1	20	0.8	4.00	7.20
6	$20_{0.2}0$	$10.40_{0.2}5.40$	15	0.8	4.00	6.40
		Panel B: 50/50 Lotteries				
7	$20_{0.5}0$	8.00_1	20	1.2	10.00	8.00
8	$20_{0.5}0$	$11.00_{0.5}6.00$	15	1.2	10.00	8.50
9	$20_{0.5}0$	10.00_1	20	1.0	10.00	10.00
10	$20_{0.5}0$	$12.50_{0.5}7.50$	15	1.0	10.00	10.00
11	$20_{0.5}0$	12.00_1	20	0.8	10.00	12.00
12	$20_{0.5}0$	$14.00_{0.5}9.00$	15	0.8	10.00	11.50
		Panel C: 80/20 Lotteries				
13	$20_{0.8}0$	15.20_1	20	1.2	16.00	15.20
14	$20_{0.8}0$	$16.40_{0.8}11.40$	15	1.2	16.00	15.40
15	$20_{0.8}0$	16.00_1	20	1.0	16.00	16.00
16	$20_{0.8}0$	$17.00_{0.8}12.00$	15	1.0	16.00	16.00
17	$20_{0.8}0$	16.80_1	20	0.8	16.00	16.80
18	$20_{0.8}0$	$17.60_{0.8}12.60$	15	0.8	16.00	16.60

Table 1.1: Decision problems

Each panel in Table 1.1, if divided by the level of c, can be seen as a multiple decision list (e.g., Holt and Laury, 2002). We presented decision problems individually, instead of using a list format, to have maximum control over the individuals' information and potential reference point. The order of the lotteries was randomized across Part I and II. The position of lottery A and B on the screen (left or right) was also randomized across subjects to avoid systematic reference point effects (Sprenger, 2010).

1.2.3 Experimental procedures

Sessions were conducted in MELESSA (Munich Experimental Laboratory for Economic and Social Sciences) at the University of Munich. Each session lasted approximately one hour. Instructions were handed out in printed form and read aloud by the exper-

[17] We also included two additional choices to serve as controls for the certainty effect (Kahneman and Tversky, 1979; Andreoni and Sprenger, 2010). We analyze these decisions and the role of peers in a separate working paper.

imenter at the beginning of each session.[18,19] The experiment was computerized using zTree (Fischbacher, 2007). In total, 188 subjects participated in the main treatments of the experiment (68 in Coin, 60 in Rand, and 60 in Choice). Their average age was 24 years and roughly 65% of all participants were female. Fields of study were almost equally distributed over 20 different fields, ranging from medicine, through cultural studies to business and economics.

One choice from one part was randomly selected at the end of the experiment for payment. If Part I was selected for payment, then one decision problem was drawn for each participant. If Part II was drawn, one decision problem was selected for each and every group only. Thus, for both group members the same decision problem was payoff-relevant.[20] Subjects were paid a show-up fee of 4 Euro additionally to their earnings from their lottery choices, yielding in total an average of 15 Euro per subject.

1.2.4 Hypotheses

A large literature argues that individuals have preferences over their outcomes (payoffs) relative to others. It is usually assumed that individuals dislike payoff differences, especially falling behind others, i.e., they want to "keep up with the Joneses".[21] A widely used model of relative payoff concerns is that by Fehr and Schmidt (1999), in which individuals dislike being behind but also dislike earning more than the peer.[22]

Across Rand and Choice payoffs remain the same. Hence, *independently* of the specific

[18]The instructions of the Choice treatment can be found in appendix A.2, the instructions of the other treatments can be obtained from the authors.

[19]In every treatment, subjects were provided with an answer sheet at the beginning of Part I, which displayed every decision problem in the same order as presented in Part I and on which they could record their decisions made in Part I.

[20]To ensure credibility, one participant was randomly selected as assistant at the end of the experiment. The assistant drew one ball from an opaque bag containing balls corresponding to each part and from a second bag with balls corresponding to each decision problem. For each decision problem, the respective combination of black and white balls was put in an opaque bag and the assistant again drew one ball. Once all draws were done, payoffs were computed and subjects were paid out in cash.

[21]This literature started with Veblen (1899) and Duesenberry (1949), who argued that conspicuous consumption choices can be explained by a desire to signal a superior status, prowess or strength. A game-theoretic literature has focused on the implications of status concerns on conspicuous consumption (see, e.g., Hopkins and Kornienko, 2004) and conformity (see, e.g., Bernheim, 1994). Here we focus on ex-post payoff differences between the decision maker and his peer, and measure strategy choices of decision makers who make conditional choices for each of the two possible lotteries of the peer. Related studies on social preferences under risk (e.g., Trautmann, 2009; Saito, 2013) point out that individuals may exhibit ex-ante relative payoff concerns, i.e., dislike inequality in expected payoffs. In our setting, such concerns yield qualitatively the same predictions, since risks are perfectly correlated. By choosing the lottery of the peer, decision makers can equalize expected payoffs both in Rand and Choice.

[22]In the context of risk taking in the presence of others, whether individuals exhibit a desire to be ahead or not may depend on the situation (see Maccheroni et al., 2012, for a discussion). In our context, in which payoff differences are relatively small and the situation allows for a simple comparison with the peer, we would rather expect individuals dislike falling behind others, but enjoy being ahead. In appendix A.1.1 we propose such a model in which decision makers are loss averse with respect to the peer's outcome, and derive conditions under which peer effects are expected to occur. Note that assuming a dislike to being ahead of the peer would even strengthen the incentive to imitate the peer.

functional form of relative payoff concerns, if these are the central motive behind peer effects, these should be the same in Rand and Choice. Relatedly, in Coin, decision makers cannot condition on the lotteries of the peers. Hence, no conditioning is expected.[23] This leads to the following Hypothesis.

Hypothesis 0: *Peer effects are the same in Rand and Choice. Peer effects are weaker in Coin than in Rand and Choice.*

Note that Hypothesis 0 relies on the assumption of perfectly correlated risks. This allows for potential imitation of both risky and safe choices. In contrast, under idiosyncratic risks, relative payoff concerns à la Fehr and Schmidt (1999), may imply that choosing the safe lottery is the unique equilibrium, as shown in Friedl et al. (2014).

While the assumption of relative payoff concerns is central in the literature, recent evidence as well as a large literature in social psychology suggest that not only payoffs may matter, but that the fact that the peer makes active choices may be an important factor generating peer effects.

In particular, recent evidence suggests that relative payoff concerns depend on whether the peer makes a choice or not (e.g., Falk and Fischbacher, 2006). For example, in ultimatum and battle-of-the-sexes games, Bolton et al. (2005) show that when payoff differences are the result of a fair random draw individuals are less likely to react negatively to receiving a lower payoff. In the context of our experiment, this suggests that disadvantageous payoff differences in Rand, which are the result of a 50-50 allocation of A or B to the peer, may be disliked less strongly than in Choice. Hence, when moving from Rand to Choice, the dislike of falling behind may increase.[24] This change directly increases the weight on (negatively valued) payoff differences in the decision maker's utility, which increases the likelihood of imitation. Payoff differences in expectation, at the same time, crucially depend on how A and B relate in terms of their expected values. Thus, if the disutility from falling behind is more pronounced in Choice compared to Rand, the effect of choices by the peer, relative to allocations, should also depend on expected values of A relative to B.

Suppose the expected value of A equals that of B ($f = 1$). In this case, the marginal increase in disutility from falling behind when choosing A or B is of the same magnitude. However, if lottery A yields a higher expected payoff ($f > 1$), the marginal increase is stronger in magnitude in case the decision maker chooses B and the peer chooses A than vice versa. This implies a stronger incentive to imitate A compared to B. The same

[23] In Coin payoff differences cannot be eliminated with certainty, unless the decision maker is certain about the peer's choice. While conditional choices are not expected, we could observe revisions which are driven by relative payoff concerns.

[24] In appendix A.1.2, we provide details on the following argument using the model of relative payoff concerns introduced in appendix A.1.1.

rationale applies to the case where $EV_A < EV_B$. Hence, if a dislike of being behind the peer increases from Rand to Choice, not only does imitation increase, but this increase in imitation depends on f.[25]

Hypothesis 1A: *Moving from Rand to Choice, imitation increases equally towards A and B if $f = 1$. It increases more towards A than towards B if $f > 1$ and less if $f < 1$.*

Alternatively, recent evidence on fairness considerations in risk taking (Cappelen et al., 2013) suggests that relative payoff concerns may depend on whether the peer chose to take on more or less risk. They show that individuals share less when others took on more risk, compared to when they took the same amount of risk but their luck differed. This suggests that relative payoff concerns would increase in Choice, and this increase would be stronger when the peer chooses the safe lottery B.[26]

Hypothesis 1B: *Moving from Rand to Choice, imitation towards B increases more than towards A.*

An extensive literature on social comparisons in social psychology proposes a different mechanism through which peer choices might be important. According to Festinger (1950, 1954), humans have a drive to evaluate their own opinions and own attitudes. In the absence of an objective, non-social measure, individuals measure the "correctness" of their own opinions and own attitudes by comparison with others.[27] When there are discrepancies between the attitudes of individuals in a group, Festinger predicts that individuals will reduce these discrepancies, either by communicating with others (influence) or by changing their attitudes towards those of the group (conformity). Festinger (1954) also argues that the strength of the influence of others will depend on how divergent their situations are from the individual's situation. The closer others' situation is, the more likely it is to be an important anchor for the evaluation of "correctness". Empirically, there is a wide range of evidence in support of these predictions in studies in social psychology (for a review see, Cialdini and Trost, 1998, and Cialdini and Goldstein, 2004).

[25] There are potentially different ways to model how relative payoff concerns are altered in Choice. For example, one could introduce a weighting factor in Choice, to increase all parameters that define relative payoff concerns as given in Fehr and Schmidt (1999). Qualitatively, it still increases the importance of f. In appendix A.1.2 we show that predictions remain the same as long the decision maker suffers more from being worse off than suffers from being better off. One may also assume that λ decreases instead of increases. Though not as intuitive, our predictions on comparative statics with respect to f would also hold in this case, although in opposite directions.

[26] We should additionally note that the effect outlined in Hypothesis 1A would also play a role here. In particular, it would imply that the increase in imitation towards B would be more dependent on f than the increase in imitation towards A. In particular, for $f = 1$, imitation towards B would increase significantly more than towards A. This difference would increase even further when $f > 1$, and would be smaller when $f < 1$.

[27] According to Festinger, "an opinion, a belief, an attitude is "correct", "valid", and "proper" to the extent that it is anchored in a group of people with similar beliefs, opinions and attitudes." Festinger (1950).

Our treatments can be interpreted as changing the social anchor. First, in Choice, the decision maker can condition his choice on the choice of his peer, i.e., the peer's choice is the social anchor. Second, in Rand, the decision maker can condition his choice on the lottery allocated to the peer. Hence, the situation of the peer is less similar and can be seen as a weaker social anchor. Though this type of "difference" is not directly discussed by Festinger, if we apply the concept of divergence in terms of the situation of the peer, we would predict a weaker influence of the peer in Rand.[28,29] This should lead to more imitation in Choice, independent of lottery characteristics. The increase should be symmetric with respect to the two available options, A or B, and should not differ depending on f.[30] We close this section with our last hypothesis.

Hypothesis 1C: *Moving from Rand to Choice, imitation towards A and B increases equally, and does not depend on f.*

1.3 Results

1.3.1 Decisions in Part I

We start with a brief review of decisions in Part I. We find no significant differences across treatments in individual decisions in Part I, as expected. Table 1.2 describes the average frequency with which A was chosen, over all decisions, by first and second movers, respectively, in each treatment. First movers choose A on average between 17.3% and 23.3% of the time, second movers choose A between 17.0% and 22.5% of the time. The Mann-Whitney (MW) tests reported in the bottom part of Table 1.2 reveal that the differences are not significant.

	% of A choices in Part I	
	First Mover	Second Mover
Coin	17.3%	19.9%
Rand	20.2%	21.7%
Choice	23.3%	17.0%
Mann-Whitney test, p-values:		
Coin vs. Rand	0.7431	0.7458
Coin vs. Choice	0.3622	0.4139
Rand vs. Choice	0.5607	0.3783

Table 1.2: Average frequency of A choices in Part I

[28]When discussing the effect of similarity with the other's situation, Festinger illustrates his argument using as an example the case of a college student who is not likely to compare himself to immates from an institution for the feeble minded to evaluate his own intelligence (Festinger 1954, p.120).

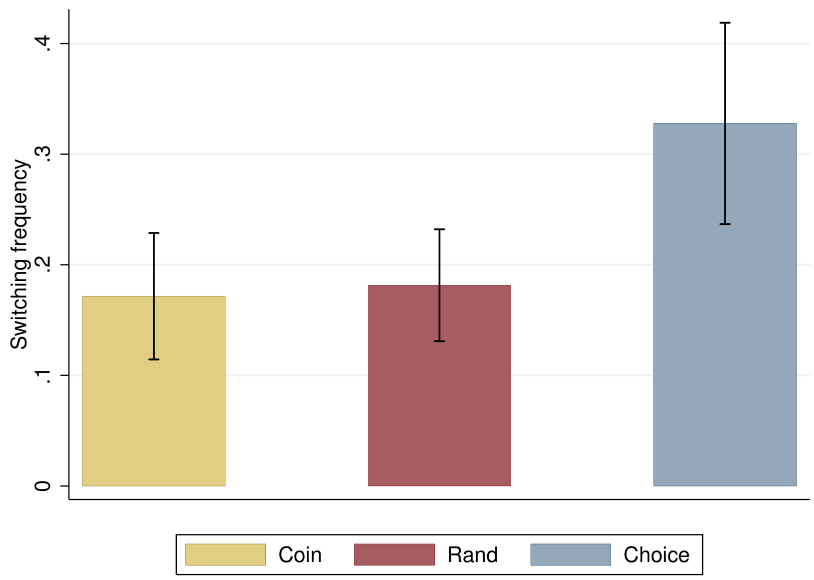
[29]In the Coin treatment, the decision maker can condition his choice on the outcome of a coin toss by the peer. In this case, the anchor is most distant from the decision maker's choice, as it is unrelated to his choice or payoffs.

[30]See appendix A.1.3 for a straightforward model based on social comparison theory.

Choices in Part I display a strong variance depending on the decision problem. If B has a lower expected payoff ($f > 1$), a vast majority of decision makers chooses lottery A when $p = 0.2$ (88.8% and 71.1%). This frequency drops to 19.7% and 20.6% when $p = 0.5$ and to 17.5% and 16.2% when $p = 0.8$. Instead, when B has a higher expected payoff ($f < 1$), it is chosen in the majority of all cases. In the intermediate cases, where A and B have the same expected payoff ($f = 1$), the frequency with which A is chosen again varies from over 26% when $p = 0.2$ down to 7.0% when $p = 0.5$.[31] Hence, on average decision makers are risk averse, as is usually observed in experiments.[32]

1.3.2 Peer effects by treatment

Figure 1.1 compares the average frequency with which decision makers switch with respect to Part I. As defined above, a switch is a change in lottery choice with respect to Part I, for at least one of the two potential choices made by the peer.



Note: Switching takes value 1 if the second mover changes his choice in Part II for at least one of the possible choices of the first mover with respect to the choice made in Part I for the same decision. Error bars in Figure 1 (a) represent ±1.645 SE, a 90% confidence interval.

Figure 1.1: Peer effects by treatment

In Coin, subjects switch in 17% of the cases, while they switch in 18% of the cases in Rand. This difference is not significant (MW-test, p-value=0.53). The switching frequency differs

[31] A detailed overview of choices in Part I is provided in Table A.1 in appendix A.3.1.

[32] We also controlled for consistency of decisions in Part I. If we assume that subjects have CRRA preferences and given the design of our lotteries, we can classify second movers as consistent or inconsistent decision makers. We find across different probability panels, controlling for certainty, that at most 15.4% of decisions patterns are inconsistent. If we exclude inconsistent decision makers from our sample our results remain qualitatively the same.

significantly - goes up to 33% - in Choice (MW-test, p-value=0.03 compared to Coin and 0.07 compared to Rand).[33] Hence, peer effects are significantly larger in Choice, than in Coin and Rand. This leads to Result 1.

Result 1. *Peer effects are significantly stronger in Choice than in Rand. Peer effects do not differ significantly in Rand and Coin.*

Based on Result 1 we reject Hypothesis 0. Peer effects in risky choices are significantly different when decision makers can condition on the peer's choices, relative to allocated lotteries and unrelated peer actions. Surprisingly, peer effects are not significantly stronger when decision makers can condition on the peer's allocated lotteries relative to her unrelated acts.

To examine where peer effects stem from, we examine the strategies adopted by decision makers when switching. Figure 1.2 displays the frequency with which decision makers choose to (1) imitate the peer, (2) deviate from the peer or (3) revise their choice from Part I (irrespective of the peer).[34]

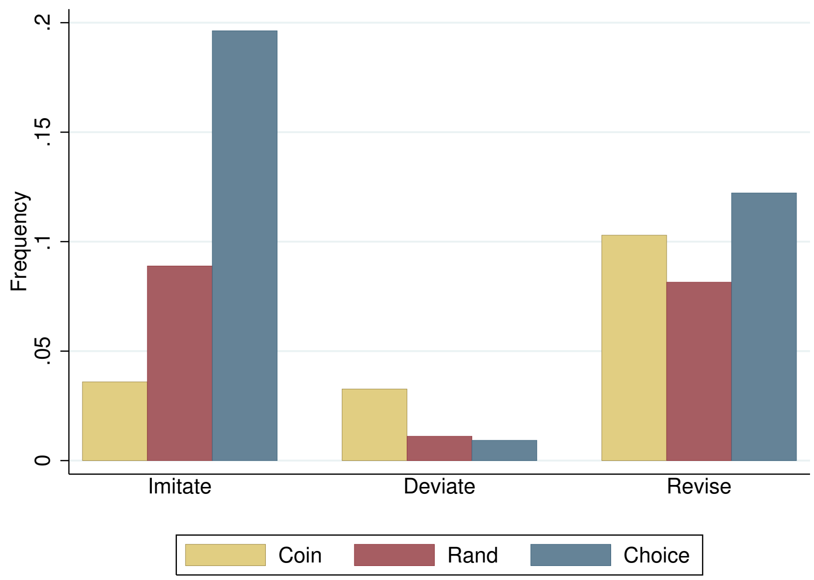


Figure 1.2: Strategy choices by treatment conditional on switching

Not surprisingly, given the lack of a link between the unrelated act of the peer and her lottery choice in Coin, the frequency of imitation (3.6%) is similar to that of deviation (3.3%). In comparison, in Rand, the frequency of imitation increases to 8.9% and that

[33] At the individual level, the distribution of switching rates also differs across treatments. It is significantly different in Choice, compared to Rand and Coin (Kolmogorov-Smirnov test, p-value=0.02 compared to Coin, p-value=0.09 compared to Rand). But does not differ significantly across Rand and Coin (Kolmogorov-Smirnov test, p-value=0.96). Figure A.1 in appendix A.3.1 displays the distribution of switching rates by treatment.

[34] Table A.2 in appendix A.3.1 displays the frequency of each strategy choice for each decision, by treatment.

of deviation decreases to 1.1%. The increase in imitation is marginally significant as displayed in Table 1.3, column (1), based on a multinomial logit regression. Interestingly, adding the frequency of imitation and deviation reveals that decision makers condition their choice on the peer in 6.9% of the cases in Coin and in 10% of the cases in Rand. This difference is not significantly different, as shown in column (4) of Table 1.3. Hence, while decision makers do not condition their choices more frequently in Rand than in Coin, when doing so, they adopt the strategy of imitating significantly more often.

In Choice imitation is significantly more frequent, and occurs in 19.6% of the cases. As shown in Table 1.3, column (1), the likelihood of imitation increases significantly in Choice relative to Coin. Further, it also increases significantly with respect to Rand (t-test, p-value=0.0582). In turn, decision makers are significantly more likely to make conditional choices (column (4)) in Choice than in Coin (t-test, p-value=0.021) and Rand (t-test, p-value=0.0646).

In contrast to imitation, deviation (column (2) of Table 1.3) and revisions (column (3) of Table 1.3) are not significantly affected by Rand and Choice, relative to the decision to stay with Part I choice. Two lottery characteristics influence the decision to revise: (i) if the lottery has a probability of 0.5, the likelihood of revising decreases, and (ii) if the expected value of A is equal or smaller than that of B ($f \leq 1$), it increases.

The findings so far are summarized below.

Result 2.

a) *Conditional choices are significantly more frequent in Choice than in Rand and Coin. At the same time, conditional choices are not more frequent in Rand than in Coin.*

b) *Imitation is significantly more frequent in Choice than in Rand and Coin. Imitation is also more frequent in Rand than in Coin.*

Hence, when decision makers can condition their choices on peers we mainly observe an increase in imitation, relative to when they can condition on their allocated lotteries or unrelated act of the peer. At the same time, on the "intensive" margin, for those decision makers who condition, imitation is the most frequently used strategy in Rand, but not in Coin. Thus, the results so far reveal that relative payoff concerns are present when the peer is allocated a lottery. However, actions of the peer matter in addition to their effect on payoffs. In what follows we investigate the increase in imitation when the peer makes active choices and examine whether it is consistent with Hypotheses 1A, 1B or 1C.

Before doing so, we briefly address switching by the peer in our empirical analysis. Peers on average switch in 12.9% of the cases in Coin, 48.0% in Rand and 11.7% in Choice. The switching rate is close to 50% in Rand, since lotteries are randomly assigned to the peer with probability 0.5. Switching does not differ significantly across Coin and Choice (MW-test, p-value=0.6962).

	(1)	(2)	(3)	(4)
		Strategy choice		Likelihood of
	Imitate	Deviate	Revise	conditional choice
Rand	0.088*	-0.022	-0.027	0.042
	[0.051]	[0.016]	[0.028]	[0.057]
Choice	0.170***	-0.023	0.016	0.132**
	[0.058]	[0.017]	[0.031]	[0.059]
$p = 0.5$	-0.01	-0.001	-0.072***	-0.012
	[0.020]	[0.008]	[0.024]	[0.020]
$p = 0.8$	-0.002	-0.007	-0.009	-0.009
	[0.022]	[0.008]	[0.018]	[0.022]
$f = 0.8$	0.017	0.014**	0.098***	0.032**
	[0.016]	[0.007]	[0.017]	[0.016]
$f = 1$	0.021	0.012	0.070***	0.035**
	[0.016]	[0.008]	[0.016]	[0.017]
Certainty	0.002	0.001	-0.004	0.004
	[0.010]	[0.006]	[0.010]	[0.012]
N		1692		1692
Pseudo-Loglikelihood		-1163.39		-599.53
Pseudo-R2		0.0697		0.0464

Note: This table presents estimated marginal effects from a multinomial logit regression on the strategy choice, taking no change as the base outcome, in columns (1) to (3), and marginal effects from a logit regression on the decision to condition on the peer (imitate or deviate) in column (4). Rand and Choice denote dummies for each treatment, where Coin is the omitted category. The variables $p = 0.5$ and $p = 0.8$ refer to the lotteries with these probabilities, taking $p = 0.2$ as omitted category. $f = 0.8$ and $f = 1$ are dummy variables for the expected value of A versus B, as defined in Table 1.1. Certainty takes value 1 if lottery B is degenerate, 0 otherwise. All regressions include individual characteristics as controls: gender, a dummy for business or economics major and age. Standard errors are presented in brackets and clustered at the individual level. ***, **,* indicate significance at the 1%, 5% and 10% level, respectively.

Table 1.3: Determinants of strategy choices in Part II

1.3.3 Imitation

Figure 1.3 below displays the average frequency of imitation towards A on the left-hand side, and towards B, on the right-hand side. Within each chart, imitation is divided by f and treatment.

The first two main features of imitation in the data are that (i) the frequency of imitation towards B is on average higher than that towards A, both in Rand and Choice, and (ii) moving to Choice the average rate of imitation increases towards both A and B. To examine the alternative hypotheses 1A–1C in detail, we regress the likelihood of imitation on treatment, on the lottery held by the peer, and on lottery characteristics, including separate regressions depending on f. Results are presented in Table 1.4.

According to Hypothesis 1A, when $f < 1$, imitation towards B should increase more strongly than that towards A. If $f > 1$, we would expect the opposite to occur. And, if

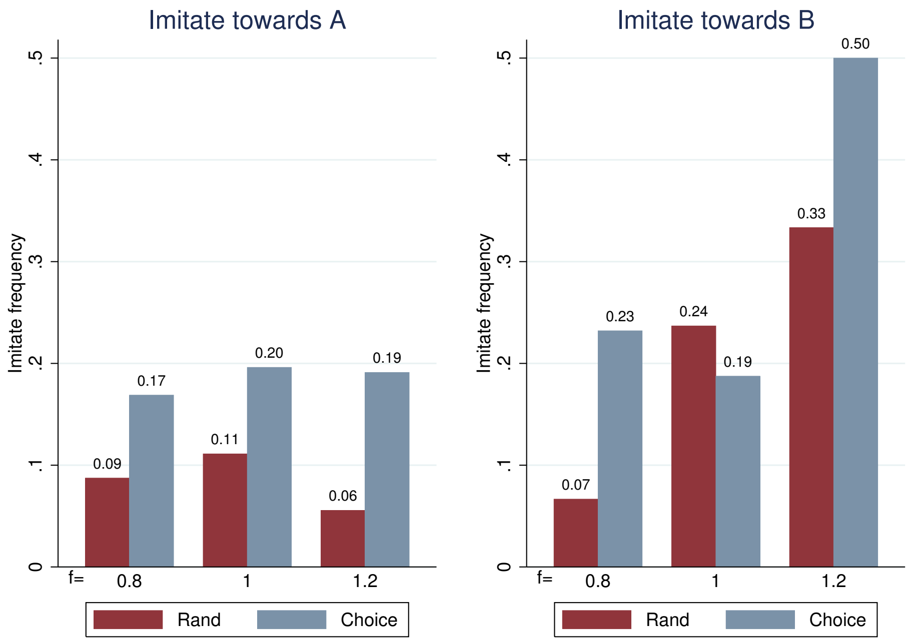


Figure 1.3: Imitation towards A and B, by f and treatment

$f = 1$, we would expect no difference in the increase. The results are presented in columns (2), (3) and (4) of Table 1.4. The interaction between Choice and imitation towards B is only significant, and negative, when $f = 1$, contrary to Hypothesis 1A.

To test Hypotheses 1B we consider the interaction term between Choice and imitation towards B over all choices (column (1)). This term is not statistically significantly different from zero, hence, not consistent with Hypothesis 1B. Further, considering effect of f on imitation, we find that the increase in imitation in Choice is similar for all f, except for when $f = 1$ and imitation is towards B. Hence, in a majority of the cases the increase in imitation does not depend on f, but not all. This is partly consistent with Hypothesis 1C. This leads to Result 3.

Result 3.

a) Imitation is on average more frequent towards the safe lottery B than towards the risky lottery A, in Rand and Choice.

b) The increase in imitation in Choice is not significantly different towards B than towards A.

c) If $f = 0.8$ and $f = 1.2$, the increase in imitation towards B in Choice is not significantly different from that towards A. If $f = 1$, the increase in imitation towards B is significantly weaker than the increase in imitation towards A.

To sum up, the evidence reveals that the increase in imitation in Choice is not significantly stronger towards the safe lottery, nor does it feature the comparative statics with respect to

	Probability of imitation			
	(1)	(2)	(3)	(4)
	All choices	If $f = 0.8$	If $f = 1$	If $f = 1.2$
Choice	0.134**	0.079	0.123*	0.180**
	[0.065]	[0.072]	[0.074]	[0.073]
Towards B	0.134***	0.098*	0.173***	0.179
	[0.037]	[0.057]	[0.051]	[0.117]
Choice * Towards B	-0.061	0.053	-0.222**	-0.044
	[0.048]	[0.077]	[0.107]	[0.122]
$p = 0.5$	0.019	0.115***	-0.003	-0.045
	[0.031]	[0.042]	[0.045]	[0.041]
$p = 0.8$	0.023	0.112***	-0.01	-0.01
	[0.032]	[0.040]	[0.040]	[0.042]
Certainty	0.008	-0.01	0.019	0.009
	[0.014]	[0.026]	[0.024]	[0.018]
$f = 0.8$	-0.016			
	[0.025]			
$f = 1$	0.014			
	[0.024]			
Observations	1080	360	360	360
Pseudo-loglikelihood	-414.34	-135.06	-148.23	-119.95
Pseudo-R2	0.0635	0.0915	0.0474	0.1281

Note: This table presents estimated marginal effects from logit regressions on the probability of imitation. All independent variables are defined as in Table 1.3. All regressions control for individual characteristics: gender, a dummy for business or economics major and age. The estimated marginal effects remain with the same sign and similar in size, if we use OLS regressions to control for potential biases in the sign of the interaction effect (see Ai and Norton, 2003). Standard errors are presented in brackets and clustered at the individual level. ***, **,* indicate significance at the 1%, 5% and 10% level, respectively.

Table 1.4: Determinants of imitation

f that would have been expected should the disutility from falling behind have increased. These facts are broadly in line with Hypothesis 1C. However, these are indirect tests of Hypotesis 1C, based on the aggregate data. A further test of Hypothesis 1C can be provided by structurally estimating a model of preferences that incorporate a social utility term. This enables us to use all individual decisions and test parameter restrictions across treatments.

In particular, the evidence so far suggests that (a) assuming decision makers derive a constant utility from conforming to other's behavior (independent of payoffs), we should observe an increase in this utility from Rand to Choice, and (b) assuming relative payoff concerns change in Choice, we should observe a change in the parameters governing relative payoff concerns from Rand to Choice. We test these conjectures by structurally estimating two models of social utility. All details about what follows can be found in appendix A.3.2. In our estimation we assume the decision maker to exhibit utility that is additively separable into consumption and social utility. In a model of relative payoff concerns, we assume that negative payoff differences with respect to the peer enter negatively into utility and are weighted by a "social" loss aversion parameter $\lambda \geq 0$, while positive payoff differences enter positively and have a weight of one. In a model based on social comparison theory we assume a constant utility γ from conforming to the peer's choice. Based on decisions in Rand and Choice we estimate these parameters, in distinct models, and test for treatment differences in λ and γ.[35]

Our findings reveal that, in a model of relative payoff concerns, decision makers exhibit significant loss aversion with respect to their peer's payoff ($\lambda > 1$), but this disutility does not change significantly across treatments. In a model based on social comparison theory, decision makers gain significant utility from choosing the peer's lottery (γ>0). Further, this utility is significantly larger in Choice compared to Rand. In terms of goodness of fit, the model of relative payoff concerns is significantly inferior to a model based on social comparison theory. Overall, these results suggest that the substantial increase of peer effects when peers make active choices may be explained by a norm to conform to others.

1.4 Conclusion

This paper examines peer effects in risk taking. We test whether peer effects can be explained by preferences over others' outcomes or whether preference interactions also depend on others' choices, in addition to distributional concerns. Our main result is that peer effects increase significantly when peers choose among lotteries, relative to when they are allocated a lottery. This reveals that choices play a significant role, on top of payoffs. At the same time, imitation of the peer is the predominant strategy adopted by those who are affected by the presence of others. Generally, this suggests that peer effects in risk taking are explained by both relative payoff concerns and a direct preference over peer choices.

We examine two alternative explanations for why choices of peers matter. First, preferences over others' payoffs might change if peers make choices. This may result in an increase in envy, i.e., the disutility from disadvantageous payoff differences, or envy may be particularly strong when peers choose safe options. Alternatively, according to social

[35] Another approach could be to simultaneously estimate parameters defining relative payoff concerns and an additional utility from conforming to the social anchor. However, imitation (or deviation) can very generally be explained by a positive (or negative) estimate of γ as well as by $\lambda > 1$ (or $\lambda < 1$). Identifying both parameters, for both treatments, simultaneously, is not possible with our data, but would be an interesting task for future work. Another approach might be to estimate mixture models, a procedure that we applied in a previous version of this paper. Mixture models have been used to estimate risk preferences in heterogeneous populations, amongst others by Conte et al. (2011) and Harrison and Rutström (2009). However, in our setting, assuming heterogeneity with respect to whether decision makers derive a social utility or not, causes the following concern. The probability to be of a certain type enters into the log-likelihood function as a multiplicative weight of the social utility, and in this way scales the estimates of λ and γ. Moreover, it leaves one additional degree of freedom.

comparison theory (Festinger, 1954), peer choices might be perceived as a decision anchor and measure for “correctness” of individual preferences, giving rise to a norm to conform to others’ behavior. Comparative statics reveal that when moving from peer allocations to peer choices, imitation increases both towards safe and risky lotteries. It also does not vary systematically with payoff differences, as would be expected by an increase in envy. Hence, at the aggregate level, the increase in imitation when peers make choices is in line with a norm to conform to peers. Structurally estimating these models provides additional suggestive evidence for this result.

Our results contribute to understanding how peers affect risky choices, including stock market participation, investment choices and insurance purchases. They suggest that not only relative payoffs matter, but also the act of choosing between risky prospects. This can have important implications for the spread of risky choices. For example, it suggests that communicating others’ risky choices may have large consequences even in environments where all individuals are equally well informed. At the same time, it reveals that imitative behavior in risk taking is most likely to spread when peers make active choices. Hence, campaigns that give “gifts” to some individuals or endow them with a particular risky asset to leverage demand may only have limited success.

Overall, as argued by Shiller (1984), “investing in speculative assets is a social activity”. It is thus important to understand what “social” actually means, and to understand how others might shape economic decisions.

Chapter 2

Conflicting Risk Attitudes*

2.1 Introduction

Conflict is pervasive in many different kinds of groups, ranging from small and large societies to organizations and teams (Simmel, 1955; Coser, 1956). Conflict, both violent, e.g., war, and non-violent, e.g., disagreements, has very harmful economic effects. Opportunities to trade or invest are forgone when two parties cannot reach an agreement. Conflict can also lead to sabotage and destruction. Understanding when conflict is most likely to arise is especially important in developing countries, where it strongly hinders the improvement of economic and social conditions (Blattman and Miguel, 2010).

To understand why, consider that in small-scale societies with imperfect credit and insurance markets and a paucity of formal savings instruments, a dense network of relationships, many of them kin-based, governs investment behavior (Fafchamps, 2003). Examples include the joint purchase of large, indivisible capital goods (a plough, an irrigation pump); informal risk-sharing arrangements (IRSAs) in case the investment goes wrong; and gifts or informal loans to help finance an investment, often with an expectation of reciprocity. The myriad ways in which people in small-scale societies in developing countries, when it comes to their investment behavior, are tied through informal arrangements would suggest a tremendous scope for disagreement, and if not settled, for conflict. One plausible motive would be when one party is more cautious, i.e., more risk averse, than the other, so that conflict may result from disagreement about the amount of exposure to risk of the investment that parties are jointly engaged in. In this paper we examine conflict from a microeconomic perspective, focusing on the role of heterogeneous risk preferences in determining interpersonal conflicts in rural villages in Uganda.

From a theoretical perspective, conflict may be modeled as the outcome of a failed bargaining process (e.g., Fearon, 1995). In the context of farming, where investments are often made jointly by groups of farmers, bargaining situations may be at the heart of so-

*This chapter is based on joint work with Marta Serra-Garcia, Ben D'Exelle, and Arjan Verschoor.

cial tensions. Consider two farmers who face the decision of how much to invest for their farming activities, e.g., in buying a plough. Assume they will equally share the payoffs from harvesting and the investment is indivisible. A central aspect of this decision, given the uncertain weather conditions, is how much risk to take. If risk preferences are private information, each farmer may have an incentive to misrepresent them during bargaining. This may lead to failed agreements (Kennan and Wilson, 1993) and generate interpersonal conflict between the two farmers. This is especially likely to be the case if their risk preferences differ substantially. In this paper, we investigate empirically whether such a relationship between risk attitudes and conflicts exists. We ask, are two individuals with different risk attitudes more likely to suffer from interpersonal conflict?[1]

Our study focuses on a society that has historically suffered from violence among its people, the Bagisu people, in Eastern Uganda (Heald, 1998). Within this region, we collect information on interpersonal conflict among pairs of adults living in the same village. In particular, we ask whether village members get along well or not, inquiring in a sensitive manner about past conflict. Additionally, we collect information about a wide range of socio-economic variables and other characteristics of the social link between each pair of adults. Two weeks following the survey, we elicit individual risk attitudes in an incentivized experiment.

Our empirical approach is based on the examination of the relationship between conflict and risk attitudes, focusing on whether the likelihood of a conflictual relationship between two linked individuals is determined by the absolute difference in their degrees of risk aversion, controlling for other relevant individual and pair characteristics. Since the composition of rural villages cannot be exogenously changed, our results cannot be interpreted as causal evidence. However, focusing on different subgroups of the population and conducting an analysis based on random links, as detailed below, provides suggestive evidence for a particular direction of the relationship. Further, providing correlational evidence is nevertheless important for several reasons. To our knowledge, no previous study has examined the determinants of interpersonal conflict, as the focus in the literature is often on friendships or, generally, positive social ties. Second, we elicit an incentivized measure of risk attitudes, and not only relate conflict to individual socio-demographic characteristics. Third, interpersonal conflict may be at the very heart of the violent episodes that the Bagisu people often suffer. Hence, understanding its potential sources may be valuable in deterring future violence.

Our results reveal that an increase in the difference in risk attitudes between two individuals significantly increases the likelihood of conflict, controlling for as many differences in

[1] By interpersonal conflict we refer to conflictual disputes among individuals, i.e., conflicts on the micro level. Our definition of interpersonal conflict thereby abstracts from conflicts between groups in society, violent strikes or protests, and civil war. For an overview of the economic literature on civil war, for example, we refer the reader to Blattman and Miguel (2010).

other characteristics as possible, as well as for relationship characteristics. More precisely, a one unit increase in the difference in risk attitudes (where these can differ by a maximum of six units), increases the likelihood of conflict by 2 percentage points. Relative to the general frequency of conflicts in our sample, 21.5%, this corresponds to an increase of roughly 10% in the likelihood of conflict.

We find that differences in risk attitudes are significantly related to the presence of interpersonal conflicts only among kin, i.e., where blood-ties exist. At the same time, differences in risk attitudes are significantly related to conflicts among pairs of male subjects. These results are in line with the argument that bargaining among farmers may lead to conflict. As Heald (1998) reports, most farming decisions are made within families, where land was often shared through ancestors. Further, such decisions are often made by males. Such results are also in line with recent evidence from Attanasio et al. (2012), who find that close friends and relatives are less likely to form risk sharing groups if their risk preferences are different.[2]

While differences in risk attitudes could lead to conflict for the reasons stated above, the opposite could just as well be true. Individuals who experience interpersonal conflict may break off relationships, decrease their social contact and over time diverge in their risk attitudes. Our finding that the role of risk attitudes is especially important in conflicts among kin, where social relationships are relatively unlikely to break, makes such a channel appear unlikely. To nevertheless explore this possibility in further detail, we exploit the fact that individuals from different villages do not have social contact, while almost everyone within a village knows each other and, hence, has either a non-conflictual or a conflictual relationship.[3] We randomly generate links between individuals across villages and thereby simulate a distribution of differences in risk attitudes among individuals who have no social relationship. If conflict leads to the breakage of links and in turn to segregation of risk attitudes, we would expect the difference in risk attitudes among those who are randomly linked to be similar to those who have conflictual links. However, we do not find this to be the case. Differences in risk attitudes are significantly larger among individuals who experienced conflict. Further, a marginal increase in the difference in risk attitudes is significantly related to an increase in the likelihood of conflict, relative to a random link.

We explore whether the level of risk aversion, instead of heterogeneity, is directly related to conflict. For example, individuals who are more risk seeking could also be more likely to exhibit interpersonal conflict. However, we do not find that risk attitudes differ significantly on average between individuals who experienced a conflict at least once and those who never did. Additionally, between pairs with the same risk attitude, we examine

[2]To control for potential differences in the economic links between pairs, in our analysis we control for the prevalence of loans and gifts within each tie.

[3]Only in 1 pair out of 918 we find that individuals report not to know each other.

whether the degree of risk aversion relates to the likelihood of conflict. We do not find evidence for this either. Also, a potential further step would have been to investigate particular kinds of conflicts between pairs of individuals. However, among the Bagisu people in rural eastern Uganda sources of conflict are often expressed in terms of vague accusations of "witchcraft" and "theft" (Heald, 1998), making it difficult to cleanly differentiate between sources.

This paper provides novel evidence on a potential source of interpersonal conflict, namely differences in risk attitudes. Our evidence suggests that among individuals who frequently make joint economic decisions, relatives and males, the likelihood of conflict may increase with differences in their risk attitudes.[4] This finding may help us understand future conflict between groups that make joint economic decisions, including small societies, kin or teams in organizations. This paper proceeds as follows. Section 2.2 briefly reviews the most closely related literature on conflict and risk attitudes, focusing on studies on developing countries. Section 2.3 describes the design of both survey and experiment. In section 2.4 we summarize the descriptive statistics of our data with respect to socio-economic characteristics and risk attitudes, before we lay out the empirical strategy in section 2.5. Results are reported in section 2.6. Section 2.7 provides a discussion and section 2.8 concludes.

2.2 Related literature

Our work has been inspired by three distinct literatures. The first literature that has provided motivation is the accounts by sociologists that differences between individuals are likely to be at the center of conflict. For instance, Deutsch (1969) argues that "A conflict may arise from differences in information or beliefs [...]. It may reflect differences in interests, desires or values." (Deutsch, 1969, p.8). Since differences between individuals could be along various dimensions, it is unclear which attributes matter most, and under which circumstances they matter. Research in organizational science that studies conflict in teams, for example, documents that differences in demographic characteristics, such as age and ethnicity, are related differently to different kinds of conflict (e.g., Pelled et al., 1999). If individuals make joint economic decisions, differences in individual risk attitudes generate a potential for conflict. As mentioned above, in bargaining situations, in which there is incomplete information, disagreements may occur in theory (e.g., Ken-

[4] Relatedly, in the context of the intergenerational transmission of risk attitudes Dohmen et al. (2012) report that a correlation between risk attitudes of parents and their children is significantly weaker if children frequently fought with their parents. While their finding – based on a subsample of the German population and, hence, documented in an industrialized and highly developed country – is surprisingly consistent with our results, Dohmen et al. (2012)'s analysis is restricted to a very particular part of social networks, namely parents and their children. Instead, we focus on a broader category of kinship, also consider non-related individuals and control for other characteristics of social links.

nan and Wilson, 1993). In appendix B.1 we outline a very stylized model of bargaining over a risky investment and show that disagreements over the amount to invest, which can be interpreted as conflict, arise when rather risk seeking individuals make proposals to very risk averse individuals. Experimental evidence on bargaining over lotteries has indeed shown that disagreements occur frequently between individuals with opposite risk preferences (Roth et al., 1988).[5]

The second literature that has motivated our study is that on risk sharing situations. From a theoretical perspective, risk sharing agreements are possible even if individuals have different risk attitudes (Mazzocco and Saini, 2012). However, if the agreement strongly disfavors one party relative to the other, then even though the agreement takes place, the disfavored party may hold a grudge against the other. Thus, disagreements (or even agreements) in joint decision-making under risk could translate to individuals developing a conflictual relationship, especially within closely linked groups.

The third literature that we have drawn inspiration from is that on the determinants of civil conflict. Conflict is especially pervasive in developing countries and a large literature has emerged with the aim of understanding its sources. One strand of the literature finds that increases in inequality and polarization within a society make the emergence of conflict more likely (e.g., Esteban and Ray, 1994, 2011).[6] Although measures of inequality and polarization are mainly defined in terms of income, Esteban and Ray (1994) suggest that other individual attributes across which people might differ should be taken into account when examining the determinants of conflict, which thus would include risk preferences.

Our study is related to a number of recent studies of risk attitudes, risk sharing and social ties. The first is the literature that examines the impact of exposure to violent conflict, war or bomb attacks, on individual risk attitudes (e.g., Voors et al., 2012; Callen et al., 2014). For example, Voors et al. (2012) find that individuals who were exposed to the consequences of civil war in Burundi are more risk-seeking. Our study differs from theirs in that we focus on interpersonal conflict between two individuals and in particular examine the relation to difference in risk attitudes of the individuals involved.[7]

[5]A potential caveat in the context of bargaining over risky investments between farmers could be that their risk preferences could be observable over time, e.g., from other risky decisions. However, if individuals fail to agree in their first bargaining situation, and this generates a significant conflict between the two parties, the information revealed ex-post may no longer be considered (as their link is then "broken").

[6]Prominent examples of other determinants of conflict in society include macroeconomic shocks – particularly variation in growth rates (see, e.g., Miguel et al., 2004) or in commodity prices (Besley and Persson, 2008; Dube and Vargas, 2013, and Bazzi and Blattman, 2014, among others).

[7]The impact of exposure to war and violence has been studied also in other regards. Rohner et al. (2013) show that ethnic conflicts in Uganda during the early 2000s had detrimental effects on trust, but fostered ethnic identity. Bauer et al. (2014) find in the Republic of Georgia and Sierra Leone that exposure to civil war during middle childhood and early adulthood significantly strengthened pro-sociality concerns towards one's in-group, but not out-groups.

A second set of studies related to our study examines risk preferences and risk sharing arrangements in poor locales, typically among small farmers. In developing countries, risk attitudes play a particularly important role due to the risky decisions that farmers face.[8] Moreover, to examine and improve risk coping strategies in the absence of formal insurance markets, risk sharing groups received considerable attention lately. Starting with Townsend (1994), several studies examined risk sharing agreements using survey data (among others, Dercon and Krishnan, 2000; Fafchamps and Lund, 2003; De Weerdt and Dercon, 2006; Fafchamps and Gubert, 2007; Karlan et al., 2009). Other studies used lab experiments in the field, for example, to examine how different enforcement mechanisms and social relationships influence the formation of risk sharing groups (e.g., Barr and Genicot, 2008; Barr et al., 2012a,b).[9] Most closely related to the present study is the paper by Attanasio et al. (2012), which examines the formation of risk sharing groups and shows that relatives and friends are more likely to form risk sharing groups. Our paper differs from them in that we examine the presence of antagonistic social relationships, and whether these are related to differences in risk attitudes.

The third related literature examines the influence of social ties on behavior. Generally, recent experimental studies have focused on friendship compared to "other" ties. In particular, several studies have examined whether individuals are more generous towards their friends than towards others, where the latter might be either anonymous, unknown people or simply not their best friends (Leider et al., 2009; Brañas-Garza et al., 2010; Goeree et al., 2010). Conflicts, though common to many relationships and with potentially detrimental consequences for outcomes in society, have not been addressed in this literature.

2.3 Survey and experimental design

We conduct fieldwork that consists of a survey of social links, followed by an experiment that elicits risk attitudes. Every subject completed both tasks. Survey and experiments were conducted towards the end of November and in the beginning of December 2012 in Sironko District, eastern Uganda.[10] For this study, we first randomly selected five

[8] Early studies studied the risk attitudes of farmers (e.g., Binswanger, 1980). Also, as argued by Lipton (1968); Norman (1974); Wolgin (1975); Schluter and Mount (1976), and Scott (1976) risk preferences of farmers might play an important role for the adoption of new technologies and agricultural practices. This is confirmed by a recent study that relates farmers' experimentally measured risk attitudes to the adoption of a superior form of cotton production (Liu, 2013).

[9] Another strand of the experimental literature has shown that group decision-making under risk and uncertainty differs considerably from individual decision-making. We refer to Conradt and List (2010) and Isenberg (1986) for an overview on group decision-making, and to Trautmann and Vieider (2011) for an overview of social interaction effects under risk and uncertainty. Group decisions may exhibit a risky shift, i.e., groups appear less risk averse than individuals, depending on the probabilities at hand.

[10] Background information on Sironko and its people can be found in appendix B.2.

subcounties from Sironko District. Within every subcounty approximately ten villages were randomly selected. For each one we took a census of households and their household members. Next, on average 20 households per village were randomly drawn to participate in the study and from these one adult per household was randomly chosen to be invited to participate in our study. We decided to invite only one adult per household to decrease potential side payments within households. In total 275 individuals from 34 villages participated in both survey and experiment.[11] In the rest of this section we first describe the survey and then provide a detailed outline of the experiment.

2.3.1 Social survey

Subjects were visited at home by trained local interviewers.[12] The survey consists of two main parts. In the first part we elicited the social links between all participants who lived in the same village as the respondent. In the second part we collected individual socio-economic characteristics.

The social links among participants within a village were elicited as follows. In an interview, the respondent was given the name and presented with a picture of one of his village members who also participated in our study. First of all, he[13] was asked whether he knew the other person. If not, we proceeded to the name and picture of the next village member on our list. If yes, he was asked, are you close friends? If the answer was no, he was asked do you get along well? Based on the last two questions we define a dichotomous variable as a measure for interpersonal conflict. This is equal to one if the respondent denied to be close friends with the other and additionally reported that they did not get along well in the past. Given that direct questions on conflicts may have adverse effects in these small societies we decided to inquire about possible conflicts among village members in a subtle and non-provocative manner. The alternative of asking a respondent directly whether or not a conflictual relationship exists was deemed as potentially disruptive by key informants who we consulted when designing the questionnaire; at the same time, they intimated that, in this local culture, respondents would answer "no" to the question "do you get along well?" to indicate that they are in conflict. Given our knowledge about the local culture and the sensitivity of conflict elicitation, we deem this therefore to be the most correct way to measure personal conflicts.

Next, we elicited the kinship relation between the respondent and the village member. They were asked whether they were related and if so, what kind of kinship existed, includ-

[11] We recruited subjects for two independent studies, both funded by the same research grant. A random share of the whole subject pool was assigned to the present study.

[12] At the time when the survey was conducted participants did not have any information or knowledge about what might happen in the experiment. However, they knew that they would be invited to participate in an experiment some weeks later.

[13] We refer to every subject as "he" throughout the paper.

ing blood relationships (parents, siblings, uncles, cousins, etc.) and affinal kin (related by marriage, i.e., in-laws). In subsequent questions, the respondent was asked whether they belonged to same social groups (including saving group, burial society, friendship group, farmers' group, microfinance group, drinking group, religious group) and whether they were neighbors. Further, they were asked whether they had given or received a loan or gift, in cash or in kind. The list of questions, in the sequence as they were asked in this part of the survey can be found in Table B.1 in appendix B.4.

In the second part of the survey we collected information on socio-economic characteristics of the respondent. These include gender, age, religion, ethnicity, and marital status. They also capture whether the respondent is the head of his household or not, his level of education and his occupation. We also asked about possible illnesses or disabilities.

The survey then proceeded to measure the household's ownership of assets, including dwelling characteristics, vehicles, livestock and land. To construct a wealth index we conducted a principal component analysis, following Filmer and Pritchett (2001).[14]

2.3.2 Experiment

Two weeks after the survey, the experiment took place. In the experiment participants' individual risk attitudes were elicited. We used the elicitation method of Gneezy and Potters (1997), in which a decision maker chooses how much to invest into a risky asset, but we framed it as a lottery choice task, for continuity with previous studies on risk taking in developing countries.[15] Starting with 6000 Ugandan Shillings (about 1.5 times the local daily wage), the decision maker chooses how much to invest in an asset that yields a net return of 100% with probability 0.8 or is lost completely, with probability 0.2. We framed this task as choosing one out of seven different lotteries which are presented in Table 2.1.

Assuming CRRA preferences, choosing lottery A, for example, implies a higher degree of relative risk aversion than lottery B or C. Hence, lottery choices serve as an ordinal measure for individual risk attitudes. Translated into values from 1 (A) to 7 (G), this measure covers a wide range of risk aversion.[16]

[14] In particular, the wealth index is determined by a principal component analysis, based on the number of rooms in the household's dwelling; the material the floor is made of (e.g., earth and cow dung or cement); the main source of lighting in the dwelling (e.g., electricity or different forms of lanterns); the number of indigenous, exotic and crossed cattle; the number of goats; the total size of land owned by the household; the number of vehicles owned by the household, thereof bicycles and motor vehicles; the number of durable goods such as generators, stoves, sofas, beds, radios, televisions, jewelry, watches, phones, and household appliances; and the number of equipment owed by the household, i.e., storage facilities, livestock stalls, watering cans, insecticide pumps, coffee pulping machines, wheel barrows, and animal pulled ploughs.

[15] Examples of studies on risk taking in developing countries, that use a similar method include Binswanger (1980); Henrich and McElreath (2002); Attanasio et al. (2012).

[16] In appendix B.5, we elaborate on difficulties in using alternative measures of risk attitudes, such as certainty equivalents, instead of direct choices.

Lottery	High outcome $p = 0.8$	Low outcome $p = 0.2$	μ	σ	CRRA range
A	6,000	6,000	6,000	0	8.13 to infinity
B	7,000	5,000	6,600	800	2.69 to 8.13
C	8,000	4,000	7,200	1,600	1.55 to 2.69
D	9,000	3,000	7,800	2,400	1.03 to 1.55
E	10,000	2,000	8,400	3,200	0.70 to 1.03
F	11,000	1,000	9,000	4,000	0.38 to 0.70
G	12,000	0	9,600	4,800	- infinity to 0.38

Table 2.1: Lotteries

Note: Amounts stated in Ugandan Shillings (UGX); 1000 UGX $\approx$ 0.39 USD (as of October 14, 2013). μ is the expected value, σ indicates the standard deviation of the lottery. Based on expected utility theory and assuming constant relative risk aversion, the CRRA parameter r refers to a utility function $U(x) = x^{1-r}(1-r)^{-1}$. For example, F is the optimal choice for an expected utility maximizer with $0.38 \geq r \geq 0.70$.

Each lottery was described to participants verbally and graphically (see Figure B.1 in appendix B.3.1). For example, for option A a green and a white counter were both worth 6,000 UGX; for option D a white counter was worth 9,000 UGX and a green counter was worth 3,000 UGX.[17] Subjects were then asked to choose one lottery. Before doing so, each subject was asked to answer four control questions.[18]

At the beginning of each session participants were informed that they would be able to earn money and that their decisions were confidential. Then everyone was asked to take a seat in the meeting room. Chairs were arranged in the room such that no subject could see what another subject was looking at. At the end of the experiment, draws were made, using a bag with counters, and participants received their payments in private.[19] Overall, 15 sessions were conducted with on average 18-19 subjects and maximally 22 subjects. In each session all participants belong to the same subcounty, but could come from different villages. In total 275 subjects participated, of whom 252 correctly answered the control questions. Only these subjects are included in the analyses below.[20]

[17]We also also showed them real money while explaining the value of each counters and demonstrated how counters would be drawn out of a bag.

[18]For example, "If you chose option C, how much would you go home with if you picked a white counter out of the bag?". The complete instructions can be found in appendix B.3.1.

[19]After this choice task, subjects participated in a second task, independent of the first one; details can be found in appendix B.3.2.

[20]We excluded 23 participants. These people only had little education (primary education or less), only about two years of schooling on average. About 78% of them were female. Table B.2 in appendix B.4 provides the above individual characteristics for both analyzed and initial sample. While these do not differ substantially, we control for individual characteristics in our analysis. Comparing the subsample which was dropped for the analysis to the analyzed sample we find that the proportion of female is larger (Fisher exact test p-value 0.004); the frequency of being married is smaller (Fisher exact test p-value 0.029); the number of years at school is smaller (MW-test p-value < 0.01); and the median age is higher (MW-test p-value 0.035). Household size (MW-test p-value 0.119) and differences in occupation (Fisher exact test p-value 1.000 (for primary occupation), 0.265 (for general farming activities)) are not significantly different. Further, we do not find that the analyzed sample and the initial sample which

2.4 Descriptive statistics

This section provides the descriptive statistics of our data with respect to individual socio-economic characteristics (section 2.4.1) and behavior in the experimental task (section 2.4.2).

2.4.1 Socio-economic characteristics

Table 2.2 presents the summary statistics of the participants' socio-economic characteristics. Forty-nine percent of participants were female. Age varies from 18 to 70, the average participant being approximately 40 years old. Sixty-one percent were heads of their household, eighty-one percent were married, and the average household consisted of about six members.[21] A vast majority earned most of their income by farming activities. In fact, ninety-six percent were involved in farming, though not necessarily as their primary occupation. Around seventy percent attended only primary school. Catholicism and Protestantism are the most prevalent religions, both practiced by nearly forty percent of our participants.

2.4.2 Risk attitudes

In the experiment subjects were asked to make exactly one decision. They choose one out of the seven lotteries given in Table 2.1. Table 2.3 reports the distribution of choices observed in the analyzed sample. 22.6% chose lottery E which paid (a salient amount of) 10,000 UGX with 80% chance and 2,000 UGX with 20% chance. Only 9% chose D, while a similar fraction of 14.7% and 13.1% chose the extremes A (very risk averse) and G (very risk loving), respectively.

Hence, we observe considerable heterogeneity in individual risk attitudes. To test whether risky choices can be explained by individual characteristics, we run an ordered logit regression to predict the likelihood to choose a particular lottery based on individual characteristics as potential determinants of risk attitudes. As reported in Table 2.4 we find that the exogenous variables gender, age and religion have a significant impact on risk tolerance.

Interestingly, we find that men are significantly more risk averse than women. This result is in line with existing studies in small scale societies, which found a tendency (though not significant) towards higher risk aversion for men, e.g., Henrich and McElreath (2002) or Gneezy et al. (2009). As expected, we find that age is negatively correlated with risky

also includes subjects who failed in the control tasks differ significantly with respect to risk attitudes. Subjects excluded from the analysis were on average slightly more risk averse, though not significantly so (MW-test on the average choice, p-value 0.107).

[21] In the visited villages it is possible that more than one person in the household takes on the role of the household head.

	Mean	Std. Dev.
Gender	0.49	0.50
Age	40.23	13.36
Household Head	0.62	0.49
Married	0.81	0.39
Number of people in household	6.04	2.77
Farming as primary occupation	0.85	0.36
Farming activities	0.96	0.20
Education (type)	1.13	0.57
Years of schooling	5.21	2.87
Education type	Freq. (%)	
Primary	70.2	
Secondary	19.1	
Tertiary	1.6	
None	9.1	
Religion	Freq. (%)	
Catholicism	38.9	
Protestantism (Anglicanism & other)	39.3	
Islam	11.5	
Born Again	9.9	
Seventh Day Adventists	0.4	

Table 2.2: Summary statistics

Note: These summary statistics refer to $N = 252$ subjects who participated in the social survey and the experiment.

	Lottery						
	A	*B*	*C*	*D*	*E*	*F*	*G*
Frequency	37	31	36	23	57	35	33
in %	14.68%	12.30%	14.29%	9.13%	22.62%	13.89%	13.10%

Table 2.3: Distribution of risk attitudes

Note: Absolute and relative frequency of elicited risk attitudes in the experiment.

choices in the experiment, meaning that older participants are significantly more risk averse than younger people. This result is consistent with other studies, e.g., Binswanger (1980), who finds that younger people exhibit relatively more risk seeking behavior. Also, Henrich and McElreath (2002) report that age had a negative, though not significant impact on the willingness to take risk in Chile and Tanzania.

In our sample, Protestants are significantly more risk seeking compared with other Christians or Non-Christians. This is in line with findings in Dohmen et al. (2011) based on the German SOEP panel, who report that Catholics, other Christians and Non-Christians are significantly less willing to take risks compared to Protestants. However, evidence on the relationship between risk attitudes and religion is mixed. In two representative samples of the Dutch people, for example, Noussair et al. (2013) find that Protestants are more risk averse compared to Catholics, but Renneboog and Spaenjers (2012) report that Catholics are significantly more risk averse than non-religious people, while Protestants are not. Regarding other individual characteristics, we do not find that individual risk attitudes are significantly correlated to years of schooling, marital status and the wealth index.

	Risk Attitudes	
	Coefficients	Odds Ratio
Gender	0.396*	1.486*
	[0.211]	[0.314]
Age	-0.0186*	0.982*
	[0.0103]	[0.010]
Protestant	0.590**	1.804**
	[0.237]	[0.427]
Years of Schooling	-0.0257	0.975
	[0.0365]	[0.036]
Married	-0.244	0.784
	[0.494]	[0.387]
Wealth index	0.0735	1.076
	[0.0817]	[0.088]
Observations	252	
Log-lik.	-474.2	

Table 2.4: Ordered logit regression

Note: This table reports the coefficients and odds ratios of an ordered logit regression. The dependent variable refers to the lottery choice and takes on values from 1 to 7, with 1 being the most risk averse and 7 the least. Gender, Protestant and Married are dummy variables taking on value one for females, protestants (Anglicans or others) and married subjects, respectively.
Standard errors are clustered on the session level and reported in brackets; *** p<0.01, ** p<0.05, * p<0.1.

2.5 Empirical strategy

This section first describes how we construct a dataset of links (dyads) between different participants (section 2.5.1). Then, we specify the regression approach (section 2.5.2) and provide summary statistics regarding the specific links in our sample (section 2.5.3).

2.5.1 Classification of links

In the social survey we elicited each participant's links to any other participant from the respondent's village (as described in section 2.3.1). Based on all interviews we construct a dataset in which each observation refers to two respondents, i and j, whose link was documented in the survey. In the following we refer to one observation as a *dyad* and to the dataset as the *dyadic dataset*.

We categorize a link between i and j either as a *no-conflict link* or a *conflict link* to distinguish between those village members who get along well and those who experienced conflicts.[22] We apply the "or-matching", i.e., define a *conflict link* to exist if either i or j (or both) indicated a conflict in the social survey. Table B.3 in appendix B.4 provides details on the distribution of *conflict links*, with respect to kin, generation, and gender.

Next, based on i and j's risk attitudes, in the following denoted by RA_i and RA_j, respectively, we define the difference in their risk attitudes, δ_{ij}^{RA}, as the absolute distance between RA_i and RA_j.

Beyond that, for each dyad ij, we observe the individual socio-economic characteristics of i and j (as outlined in section 2.4.1) as well as other characteristics of the link between i and j. More specifically, based on the socio-economic survey we measure absolute distances in age (in years), wealth (wealth index) and education (ordered categories of primary, secondary, tertiary school); we also code differences in gender, marital status and occupation, whether they belong to different ethnic groups, and whether they differ in their ability to work.

In addition to differences in socio-economic characteristics, the social survey collects further information about a link, which we include in our analysis: whether i and j are neighbors, belong to the same social groups, received or offered a loan and/or gift to the other one, and whether they are kin.[23] Our definition of kin captures a broad measure for being related. Not only close relatives, i.e., parents and their children, but also relatives over two generations, such as grandparents and their grandchildren or cousins, are classified as kin. Although people in our sample might be more likely to be related compared to developed countries, kinship constitutes an important component within the social network of Ugandan people (as in other African rural societies). Kin live in the same or neighboring households, hence, sharing proximity and time. Moreover, families share and jointly utilize land, which is passed on from generation to generation.

2.5.2 Model specification

In the data analysis we estimate logit models based on our dyadic dataset, to regress the likelihood of a *conflict link* on differences in individual characteristics as well as characteristics of links.[24] Summary statistics of the covariates can be found in Table B.4 in

[22] As noted earlier, only one pair of individuals agreed to not know each other, in which case we would say that no link exists. This observation is excluded in the analysis reported in section 2.6.1.

[23] We define the category *kin* similar to the one of *conflict*, i.e., based on the "or"-matching. This means, a kin link is assumed to exist if at least one of both respondents claimed that a kin link exists. The same applies to the classification of neighbors, group members, and exchanges of gifts and loans.

[24] These covariates control for characteristics that might be correlated with both differences in risk attitudes and the likelihood of conflict. The characteristics of social links might induce an endogeneity bias. For example, individuals who experienced a conflict might be less likely to exchange loans or gifts, or participate in the same social group. We control for this by testing whether results change if social link

appendix B.4.

We use the following notation to specify dyad ij:

$c_{ij} \in \{0,1\}$ is an indicator for a *conflict link* between i and j;

δ_{ij}^{RA} denotes the difference in i and j's risk attitudes;

Δ_{ij}^{E} is a row vector of differences between i and j's socio-economic characteristics;

Δ_{ij}^{S} is a row vector of characteristics of i and j's link;

v_{ij} denotes the village of i and j;

S_{ij} is a row vector that indicates in which experimental session(s) i and j participated.[25]

Then, the likelihood for the presence of a conflict, i.e., $c_{ij} = 1$, is assumed to be given by

$$Pr\left(c_{ij} = 1 \mid \delta_{ij}^{RA}; \Delta_{ij}^{E}; \Delta_{ij}^{S}; v_{ij}; S_{ij}\right) = F\left(\alpha + \delta_{ij}^{RA}\beta + \Delta_{ij}^{E}\gamma + \Delta_{ij}^{S}\kappa + v_{ij}\xi + S_{ij}\eta + \epsilon_{ij}\right), \tag{2.1}$$

where $F(x) = (1 - \exp(x))^{-x}$ denotes the cumulative standard logistic distribution function. We estimate the parameters in equation (2.1) based on a dyadic regression approach in which we cluster standard errors at the village level. We include village fixed effects and session fixed effects for both participants. Since all links (and observed conflicts) concern relationships *within* villages, controlling for village specific unobservables as well as for correlations at the village level is particularly important.[26]

2.5.3 Dyadic dataset

Our sample consists of 917 dyads; the composition of their links is summarized in Table 2.5.[27] Around one fifth (21.5%) of remaining dyads are categorized as *conflict links*.

characteristics are excluded as independent variables. This is not the case. We will refer to additional results in the appendix where relevant in section 2.6.

[25] Note that for all dyads it holds that i and j come from the same village. In contrast, i and j do not necessarily participate in the same experimental session since invitations to particular sessions were randomized at the subcounty level.

[26] Alternatively to clustering to the village level, we could also cluster in two dimensions with respect to both sessions of i and j. This approach, actually developed for panel data analysis in finance (see e.g., Cameron et al., 2011; Thompson, 2011), is not optimally suited for our dataset because the number of sessions is substantially smaller than the number of villages. But as discussed in Petersen (2009) clustered standard errors "are consistent as the number of clusters grows" (p. 440); and, hence, clustering on the village level provides the more conservative approach which also cleanly controls for within-village correlations. When conducting a two-dimensional clustering dyadic regression, the results remain qualitatively the same. Another approach would be to cluster on the subcounty level since all participants of each experimental session belong to the same subcounty. However, since our dataset only covers five different subcounties, following the argument above, the number of clusters is insufficiently small. One could also estimate dyadic ols models to regress the differences in risk attitudes on a conflicts dummy and other independent variables; again, results remain qualitatively the same.

[27] Note that we observe multiple links for each participant within his or her village. On average, for each respondent we elicited his or her link to eight other participants, respectively (standard deviation = 2.4; same for initial and analyzed sample).

Almost 40% of our sample are classified as kin. Among those, 16.5% correspond to *conflict links*, compared to 24.5% among village members who are not related. Hence, conflicts within families seem to be less likely than across families.[28]

	% of conflicts	N (ties)
All	21.48%	917
Kin		
No	24.56%	566
Yes	16.52%	351
Senior/Junior	18.59%	156
Same generation	14.87%	195
Gender		
Female-Female	20.10%	194
Female-Male	26.81%	470
Male-Male	12.65%	253

Table 2.5: Dyadic dataset

Due to random sampling nearly half of all dyads, 51.3%, are formed by one man and one woman, 21.2% by two women and 27.6% refer to two men.[29] *Conflict links* occur least frequently between men, only in 12.7% of all respective dyads. In comparison, 26.8% of mixed gender pairs and 20.1% of all female-female dyads experienced a conflict, respectively.

2.6 Results

This section examines whether differences in individual risk attitudes are related to the likelihood with which individuals experience conflicts. First, we report estimation results based on the dyadic dataset of links among village members (section 2.6.1). In a second step, we apply a simulation based approach which allows us to compare existing links (*no-conflict links* or *conflict links*) to randomly generated links across villages (section 2.6.2).

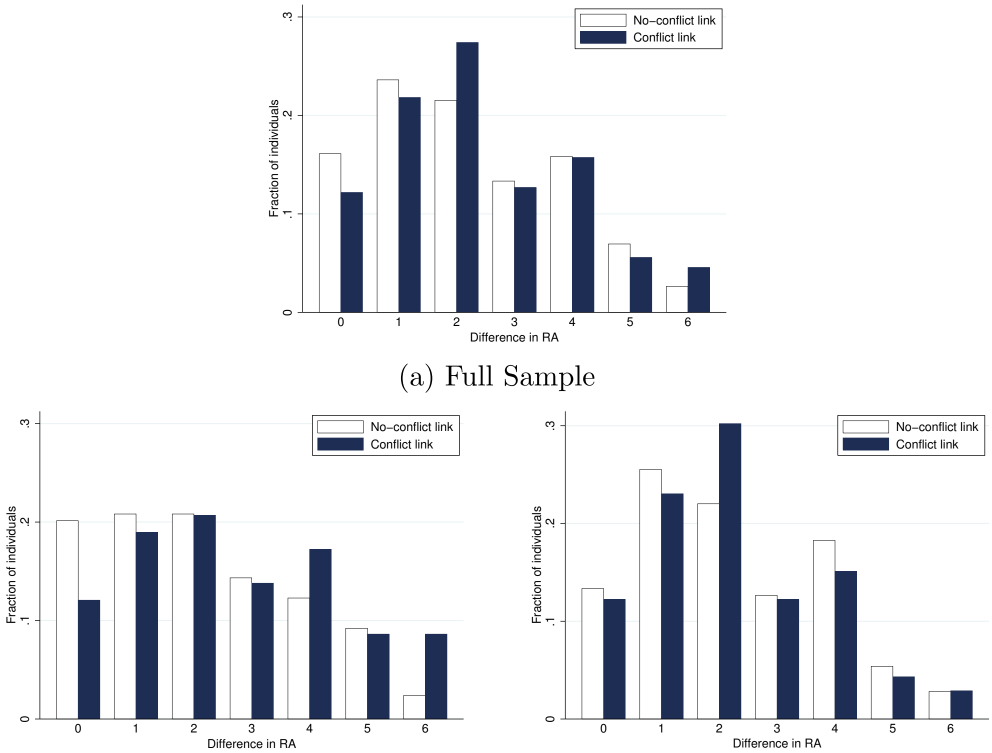
2.6.1 Conflict links and differences in risk attitudes

Figure 2.1 displays the distribution of differences in individual risk attitudes by the category of dyads, i.e., distinguishing between *no-conflict* and *conflict links.* Averaging over the sample (Figure 2.1(a)), the distribution shifts to the right for individuals who experi-

[28]The difference in the likelihood of a *conflict link* between kin and nonkin is confirmed to be significant in dyadic regression models, which we report in the subsequent analysis (section 2.6.1).

[29]We noted earlier that more women than men failed in the control task.

enced a conflict relative to those who did not. This shift is even more pronounced within kin (Figure 2.1(b)), but not apparent for non-kin (Figure 2.1(c)).



(a) Full Sample

(b) Kin

(c) Nonkin

Figure 2.1: Distribution of differences in risk attitudes

Note: These graphs display the distribution of differences in individual risk attitudes, for the analyzed sample (a), for kin (b), and nonkin dyads (c).

Our regression analysis reveals a persistent and significant influence of differences in individual risk attitudes on the likelihood of conflicts. Table 2.6 reports the estimated marginal effects based on two model specifications and different subsamples. Considering all ties, columns (1) and (2), the probability of conflict increases by 2 percentage points when the difference in risk attitudes increases by one unit. Relative to the average frequency of conflict, namely 21.5%, this corresponds to an increase by roughly 10%.[30,31] Two other individual characteristics increase the likelihood of conflict significantly, differences in age and gender. If we compare the magnitude of their effect in terms of standard deviations to that of differences in risk attitudes, we find that an increase in one standard deviation in difference in risk attitude increases the log odds ratio of conflict by 0.215. This effect is the same as the effect of increasing age by one standard deviation, 0.235,

[30]This interpretation treats the independent variable δ^{RA} in a cardinal way. To test whether this assumption is justified we ran some robustness checks and estimated logit models with dummy variables for small and large differences in risk attitudes. The results remain qualitatively the same and are presented in Table B.5 in appendix B.4.

[31]Table B.6 in appendix B.4 presents estimates from models which do not include social link characteristics as covariates. As previously noted, results do not change significantly.

Likelihood of conflict	All (1)	All (2)	Nonkin (3)	Kin (4)	Female-female (5)	Female-male (6)	Male-male (7)
δ^{RA}	0.019*	0.019*	0.004	0.048**	0.001	0.023	0.038**
	[0.010]	[0.011]	[0.013]	[0.019]	[0.028]	[0.018]	[0.016]
Age distance	0.003**	0.003***	0.003***	0.003	0.010***	0.001	0.009***
	[0.001]	[0.001]	[0.001]	[0.002]	[0.003]	[0.002]	[0.002]
Diff. Gender	0.120***	0.121***	0.098**	0.238***			
	[0.036]	[0.030]	[0.040]	[0.040]			
Diff. Ethnicity	0.111	0.057	0.114	0.235	-0.18	0.145	0.175
	[0.138]	[0.123]	[0.131]	[0.158]	[0.272]	[0.168]	[0.250]
Diff. Marital Status	0.045	0.034	0.019	-0.022	0.192**	-0.01	0.089
	[0.037]	[0.038]	[0.048]	[0.048]	[0.082]	[0.060]	[0.060]
Diff. Religion	-0.067***	-0.084***	-0.103***	-0.014	0.033	-0.143***	-0.075
	[0.024]	[0.022]	[0.031]	[0.052]	[0.068]	[0.046]	[0.091]
Education distance	0.021	0.006	-0.019	0.01	0.115	0.018	0.019
	[0.022]	[0.024]	[0.027]	[0.049]	[0.083]	[0.036]	[0.070]
Wealth distance	-0.004	-0.003	0.002	-0.005	0.062***	-0.013	-0.024*
	[0.009]	[0.008]	[0.009]	[0.022]	[0.024]	[0.015]	[0.014]
Diff. Occupation	0.033	0.035	0.038	0.043	0.04	0.034	0.025
	[0.044]	[0.039]	[0.056]	[0.056]	[0.091]	[0.061]	[0.044]
Diff. In Disabilities	0.052	0.065**	0.060*	0.089	0.026	0.053	0.033
	[0.033]	[0.030]	[0.030]	[0.053]	[0.089]	[0.032]	[0.106]
Neighbors		-0.144***	-0.190***	-0.062	-0.097	-0.139***	-0.200***
		[0.030]	[0.042]	[0.051]	[0.077]	[0.053]	[0.055]
Diff. Groups		-0.169***	-0.186***	-0.154***	-0.097	-0.206***	-0.149***
		[0.036]	[0.044]	[0.034]	[0.082]	[0.054]	[0.035]
Loan		-0.030	-0.077	0.008	-0.202*	-0.038	0.03
		[0.037]	[0.058]	[0.034]	[0.106]	[0.069]	[0.056]
Gift		-0.054	-0.060	0.075*	-0.208*	-0.081	0.011
		[0.035]	[0.048]	[0.045]	[0.116]	[0.053]	[0.060]
Kin		-0.080**					
		[0.035]					
% conflict	21.46%	21.46%	24.56%	16.52%	20.00%	26.81%	12.65%
Observations	839	839	521	251	133	403	188
Log-lik.	-372.9	-327.2	-205.7	-74.96	-46.33	-164.3	-43.83

Table 2.6: Dyadic logit regression

Note: This table reports marginal effects from a dyadic logit regression on conflict. δ^{RA} denotes the absolute difference between individual risk attitudes; *Age, Wealth* and *Education distance* refer to absolute differences in age (years), wealth index, education (primary, secondary, tertiary), respectively. Variables *Diff.* $(\cdot)$ denote dummy variables that take value 1 if individuals differ w.r.t. $(\cdot)$, and 0 otherwise. *Neighbors*, *Loan*, *Gift*, *Kin* are dummy variables that take value 1 if individuals are neighbors, exchanged a loan and gift with each other, and are kin, respectively. *Senior/Junior* takes on value 1 if relatives belong to different generations. Both regressions includes village fixed effects and session fixed effects for both individuals per dyad. Standard errors are clustered on the village level and reported in brackets; *** p<0.01, ** p<0.05, * p<0.1.

and half that of changing gender from a same-gender link to a different-gender link, 0.419. Hence, in terms of magnitude, differences in risk attitudes are important as well.[32]
In contrast, two factors contribute to significantly decreasing the likelihood of conflict: belonging to a different religion and belonging to a different social group. Both findings suggest that conflicts are likely to occur among those village members who frequently meet, for example, in religious gatherings, drinking groups or microfinance meetings. Further, interpersonal conflicts are significantly less likely within families relative to unrelated village members. Overall, different risk attitudes are significantly related to conflict.
We examine in which subsamples differences in risk attitudes are most likely to be related to conflict. First, we examine kin and nonkin dyads, in columns (3) and (4) of Table 2.6. Remarkably, the relationship between differences in risk attitudes and the presence of conflicts is the more pronounced within families, in terms of magnitude and significance. We find that one unit increase in the difference in risk attitudes increases the probability that kin experience conflicts by 5 percentage points. Further, differences in age do not significantly affect the likelihood of conflicts within families, although they still do in non-related dyads.[33]
Second, we examine whether differences in risk attitudes play a different role across different gender combinations in a dyad. Columns (5), (6) and (7) report the estimation results for three disjoint subsamples, for dyads where both individuals are female (column (5)), for individuals who are of different gender (column (6)), and both male (column (7)).[34] We find that differences in risk attitudes are significantly related to the presence of conflicts only for male-male dyads. In contrast, for pairs of mixed gender or two women, any relationship is insignificant. Moreover, women among each other seem to be concerned about different issues compared to other gender pairs. In particular, having a different marital status significantly increases the likelihood of conflict, by 19 percentage points. Conflicts between men, on the other hand, are significantly influenced by differences in age, by whether village members are neighbors or belong to the same social groups. Importantly, an increase in the difference in risk attitudes by one unit increases the likelihood of conflict by 4 percentage points between two men.

[32] The coefficients of the dyadic logit model are presented in appendix B.4, Table B.7.

[33] Given the significant correlation between age and individual risk attitudes (section 2.4.2), we ran additional estimations in which we control for whether relatives belong to the same generation, such as siblings or cousins, or to different generations, such as parents and their children or aunts/uncles and their nieces/nephews. Again, differences in risk attitudes are significantly related to the presence of conflicts, magnitudes and significance remain unchanged, but generation does not matter significantly for the likelihood of conflicts.

[34] Due to limited sample size it is not feasible to estimate regressions for subsamples that distinguish between different gender pairs as well as between kin at the same time.

Our results so far can be summarized as follows.

Result 1. *Differences in risk attitudes are significantly and positively related to conflict. This relationship is particularly strong for kin and among men.*

2.6.2 Conflict versus random links: a simulation approach

So far, we have relied on the dataset of existing links which we observed at the village level. In our data, every dyad is either in conflict or no conflict. There are no dyads (except for one) in which two individuals within a village report not to know each other, i.e., where there is no link at all. In this section we exploit the fact that individuals from *different* villages do not have a link and thereby generate *random links*, between pairs of people who do not know each other. The question we ask is, are differences in risk attitudes in links with conflict similar to links between individuals who do not know each other? If the answer is yes, this would indicate a relationship from conflict to risk attitudes, by which conflict may break a relationship and lead individuals who potentially had similar risk attitudes to evolve towards risk attitudes as different as those of people who do not know each other. If the answer is no, this would provide supportive evidence for the fact that the relationship could go the opposite way: differences in risk attitudes leading to conflict.

More precisely, for each individual we formed 100 links by randomly selecting individuals from different villages, without replacement. Appending these *random links* to our initial set of observations yields a dyadic dataset which allows for three categories of dyads. Under the assumption that social interaction mainly happens within villages, *random links* provide some insights on individuals who do not (or only rarely) know each other.[35]

Table 2.7 summarizes the differences in risk attitudes for dyads of all three categories. We find that differences in risk attitudes differ considerably in their means. The average is largest for those who experienced a conflict, and smallest for those who know each other and get along well. For *random links*, the mean difference in risk attitudes is close to the midpoint between *no-conflict* and *conflict links*.

As a next step, we ask whether an increase in the difference in risk attitude is equally likely to predict a conflict link and a random link. To do so, we estimate an ordered logit model in which we regress the likelihood to observe a particular category of dyad, i.e., *no-conflict link*, *conflict link*, or *random link*, on differences in risk attitudes and other socio-economic

[35] We could have created *random links* across different villages but within the same subcounties, in which case villages of randomly linked individuals might be more similar. To generate links across villages and subcounties, however, makes *random links* more comparable to links between individuals who do not know each other. In either case, both approaches yield results that are qualitatively and quantitatively similar.

	No conflict	Conflict	No tie
Mean difference in risk attitude	2.21	2.33	2.26
Std. Error	0.061	0.115	0.010

Table 2.7: Mean differences in risk attitudes - by category of link

Note: This table reports the average of absolute values in risk attitudes (δ^{RA}) and their standard errors by three categories of links: *no-conflict link*, *conflict link*, based on existing ties in the dataset, and *random link*, based on simulated ties.

characteristics.[36] Analogously to models estimated in section 2.6.1, we include village and session fixed effects of both individuals, and standard errors are clustered on the village level.[37] We find that differences in risk attitudes significantly increase the probability that individuals experience conflicts compared to the probability that individuals are randomly linked, as shown in Table 2.8. In contrast, an increase in the difference in risk attitudes does not imply a significant change in the likelihood for being randomly linked and for getting along well, i.e., *no-conflict.* The effects might seem small in their magnitude, but given that in the extended sample only 0.75% of dyads have interpersonal conflicts, an increase in the likelihood for such a conflict by 0.06 percentage points is relatively large and close to 8%. The findings that conflicts are significantly correlated to differences in age, gender, and religion, are once more confirmed.

Result 2. *In a sample of existing links between village members and randomly generated links across different villages, differences in individual risk attitudes are largest for conflict links. Relative to random links a marginal increase in the difference in risk attitudes is significantly related to an increase in the likelihood of conflict.*

2.7 Discussion

We find that individuals are significantly more likely to report a conflictual relationship when the difference between their individual risk attitudes is larger. This tendency is particularly pronounced within families and for male-male dyads. Further, differences in risk attitudes are significantly larger in dyads with reported conflictual links than in random dyads.

To the best of our knowledge this is the first paper to identify close interdependencies between interpersonal conflicts and risk attitudes. The finding that individuals who differ

[36]Note that information about the social relationship is not available for simulated ties, and hence, variables from the social survey are not included in these regressions.

[37]For randomly generated links, individuals i and j do not come from the same village. Hence, clustering only on i's village might not completely rule out biased standard errors. However, we address this concern by letting each individual appear as person i in at least one observation, and by including fixed effects of i *and* j's villages and sessions.

	No conflict	Conflict
Difference in RA	-0.000565	0.000647**
	[0.000593]	[0.000319]
Age distance	-0.000017	0.000100**
	[0.000078]	[0.000043]
Diff. Gender	-0.001942	0.004231***
	[0.001871]	[0.001178]
Diff. Ethnicity	-0.004858	0.003684
	[0.003610]	[0.003474]
Diff. Marital Status	-0.003244	0.001381
	[0.002269]	[0.001686]
Diff. Religion	-0.020476***	-0.005972***
	[0.005475]	[0.001726]
Education distance	-0.000612	0.000075
	[0.001503]	[0.000910]
Wealth distance	-0.000445	-0.00035
	[0.000573]	[0.000381]
Diff. Occupation	-0.003782	-0.001269
	[0.002335]	[0.002179]
Diff. in Disabilities	-0.001076	0.002768**
	[0.001529]	[0.001292]
Observations	26,117	
Category of interest as % of all observations	2.76%	0.75%
Log-lik.	-3787	

Table 2.8: Ordered logit regression: likelihood for link categories

Note: This table reports the marginal effects of an ordered logit regression. The dependent variable refers to the category of link, i.e., *no-conflict link* (column (1)), *conflict link* (column (2)); *random link* is the base category. Independent variables are defined as in Table 2.6.
Regression includes village and session fixed effects, for both individuals in each dyad. Standard errors are clustered on the village level and reported in brackets; *** p<0.01, ** p<0.05, * p<0.1.

in their attitudes towards risk are more likely to experience conflicts with each other is novel to the literature on risk attitudes as well as to the literature on the endogenous formation of risk sharing groups (both reviewed above). We close this paper with some remarks on possible ways in which risk attitudes may relate to conflict and comments on identifying correlation versus causality.

2.7.1 Risk levels or differences?

One may conjecture that conflict may be driven by levels or risk aversion, which perhaps correlate with some personality traits, and not necessarily with differences in risk attitudes within a dyad. We address this possible confound in two ways. First, we find that the average risk attitude elicited in the experiment does not differ significantly between those people who actually report a conflict at least once compared to those who never report a conflict (Fisher exact test p-value 0.297).[38] Second, we estimate dyadic logit models following the approach used in section 2.6.1. For dyads where individuals are similar in terms of their risk attitudes we test whether the level of risk attitudes (which ranges from 1 to 7) has an effect on the likelihood of a conflict to be present. Results can be found in Table B.8 in appendix B.4. We find that (i) conditional on having exactly the same risk attitudes ($\delta^{RA} = 0$), and (ii) conditional on a difference by only one unit ($\delta^{RA} = 1$), the likelihood of a *conflict link* is not significantly related to the absolute degree of risk attitudes (RA itself). Hence, Result 1 and Result 2, reported above, are not driven by the possibility that the level and not the difference in risk attitudes is related to conflict.

2.7.2 Correlation and causality

Exogenously varying differences in risk attitudes among inhabitants of the villages studied in eastern Uganda is not something we have attempted in this study. We are hence able to draw conclusions about correlations, but do not prove statements of causality. Although identifying directions of causality is an important task, randomly assigning individuals to villages or groups for the sake of obtaining an exogenous source of variation in differences in risk attitudes, and thereby to be able to observe a causal link to conflict, is clearly morally objectionable, the more so since in rural eastern Uganda interpersonal conflicts relate to serious disputes and often result in violence.

Nonetheless, we ask ourselves, which mechanism seems more likely to explain the relations observed in our data? On the one hand, individuals might experience interpersonal conflict because they differ in their risk attitudes, as suggested by existing results on bargaining under risk and heterogeneous preferences. On the other hand, individuals might differ in their risk attitudes because they experienced conflicts in the past and consequently give each other a wide berth in the future. In the long run, by breaking links opponents might join different peer groups and assimilate to their groups' (mean) attitudes. It may also be possible that both causalities might exist at the same time: differences in risk attitudes might lead to conflicts, and conflicts might even strengthen differences in risk attitudes, generating segregation with respect to attitudes and a high frequency of conflicts.

We do not find much evidence in favor of a causal effect of interpersonal conflicts on differences in risk attitudes. Further, our results are strongly driven by kinship. Belonging to the same kin, however, makes it more difficult – or more costly in economic terms – to break an existing link. If conflicts explain differences in risk attitudes, then we should expect to observe a correlation between conflict links and differences in risk attitudes not only within certain subsamples.

[38]In our sample, we find that the majority of subjects (220) actually never report a conflict with anyone. Among those who report a conflict with at least one of their peers, the frequency of reported conflicts is quite uniformly distributed over (0,1]. Only one subject reports to have conflicts with all of his peers.

2.8 Conclusion

This paper examines whether interpersonal conflict is related to differences in individual attitudes, particularly to differences in attitudes towards risk. Our fieldwork in rural Uganda includes a social survey to identify links between village members, which is followed by an experiment to elicit risk attitudes. Our sample covers nearly one thousand dyads of individuals and provides detailed information about socio-economic characteristics as well as characteristics of social relationships. With the exception of only one dyad, all village members know each other. Out of these existing links more than a fifth, 21.5%, report interpersonal conflict.

We find a persistent and significant relationship between the presence of conflict links and differences in risk attitudes: a larger difference is significantly related to a higher likelihood for interpersonal conflict. Interestingly, this relationship is particularly driven by kin, but not found to be significant for non-related village members. More precisely, for kin, a one unit increase in the difference in risk attitudes corresponds to a four percentage points increase in the likelihood of conflict, raising the frequency of conflict links within families by more than 20%. With respect to gender, we find a significant correlation only among males, in contrast to social ties between women or individuals of different gender. To extrapolate our analysis to links between individuals who are very unlikely to know each other, we use a simulation approach and randomly generate links across villages. If conflicts are likely to result in the severing of links, as a result of which differences in risk attitudes might increase, we would expect risk attitudes to be similar across random and conflict links. However, differences in risk attitudes are significantly larger among conflict links. Consistent with our previous results we find that a marginal increase in the difference in risk attitudes correlates to a significant increase in the likelihood of conflict links, relative to randomly generated links. Moreover, we do not find any evidence that risk attitudes per se are correlated to personal attributes which might be related to interpersonal conflict.

An important novelty of this paper is our focus on negative interpersonal links, conflict, instead of positive relationships, such as friendship, and to links with only little or no depth. In that sense, our paper provides the first evidence which relates differences in individual attitudes to interpersonal conflict. Our evidence suggests that among individuals who frequently make joint economic decisions, kin and males, the likelihood of conflict increases with differences in their risk attitudes. Examining which particular types of conflict relate to differences in attitudes towards risk, and how these could potentially be prevented, will be an important step for future research. Our results explicitly show how fragile interpersonal relationships might be under heterogeneity in risk attitudes.

Chapter 3

An Anatomy of Ambiguity Attitudes*

3.1 Introduction

In decision under uncertainty, risky prospects with known probabilities are often distinguished from ambiguous prospects with unknown or uncertain probabilities. Inspired by a classic article by Daniel Ellsberg (1961), it is typically assumed that people dislike ambiguity and adjust their behavior in favor of known-probability risks, even at significant costs. A large literature has studied the consequences of such ambiguity aversion for decision-making in the presence of uncertainty. Building on decision theories that assume ambiguity aversion, this literature shows that ambiguity can account for empirically observed violations of expected utility-based theories ("anomalies"). In financial economics, ambiguity aversion has been employed to explain phenomena such as the equity premium and risk-free rate puzzles (Maenhout, 2004; Collard et al., 2011; Gollier, 2011; Ju and Miao, 2012), and the stock market participation puzzle (Dow and Werlang, 1992; Easley and O'Hara, 2009). Alary et al. (2013) and Snow (2011) show that ambiguity aversion influences optimal insurance take-up, deductible choice, and self-protection activities. In health economics, Berger et al. (2013) find that ambiguity about the diagnosis or the treatment of a medical condition affects patients' treatment choices, while Hoy et al. (2014) explain the low take-up of costless genetic tests by ambiguity aversion. Ambiguity aversion has also been employed in economic models of climate change to motivate rapid emission cuts (Millner et al., 2010; Farber, 2011). Many of these results have served to motivate regulation and policy (see Farber, 2011).

These theoretical contributions presume a universally negative attitude towards ambiguity. Such an assumption seemed descriptively justified on the basis of an experimental literature following Ellsberg's original article. Many studies have implemented an urn-

*This chapter is based on joint work with Martin G. Kocher and Stefan T. Trautmann.

choice experiment proposed by Ellsberg to identify ambiguity attitudes (see section 3.2), and have predominantly found ambiguity aversion. However, as Ellsberg (2011) argues in a recent commentary at the occasion of the 50th anniversary of his seminal article, the predominance of ambiguity aversion in experimental findings might be due to a narrow focus on the domain of moderate likelihood gains, as in his original examples. While fear of a bad unknown probability might prevail in this domain, people might be more optimistic in other domains, hoping for ambiguity to offer better odds than a known-risk alternative.

Trautmann and van de Kuilen (forthcoming) review the empirical literature on ambiguity and find evidence for a more complex pattern of attitudes: while ambiguity aversion is the predominant finding in the domain of moderate likelihood gains and low likelihood losses, for moderate likelihood gains and low likelihood losses ambiguity seeking is often reported. Notable, many of the above cited theoretical contributions in economics and finance concern applications in which unlikely events and loss outcomes are important. A rejection of universal ambiguity aversion would therefore have important implications for the empirical relevance of these theoretical findings. In particular, if ambiguity seeking is predominating in important domains, theoretical analyses may fruitfully consider the implications of ambiguity loving for economics and finance.

However, a rejection of universal ambiguity aversion on the basis of the above review results should be considered with care. The basic Ellsberg paradigm is easy to implement, and has consequently been studied in hundreds of experiments. In contrast, conducting experiments with losses and identifying ambiguity attitudes for low likelihood events is complex. This has led to significant design heterogeneity across domains, which could potentially explain the differences in observed attitudes. Existing studies on these domains are much fewer, with basically no studies that consider all four domains with an identical design in terms of financial incentives and elicitation procedures (see appendix C.1 for an overview of existing studies that report on all four domains, and Table C.1 and C.2 for their design features). Given the importance of ambiguity attitudes for economic theorizing and policy, a careful measurement of these preferences across the gain and loss, and across the low and moderate likelihood domains is warranted. The current paper presents such measurements, and also studies ambiguity attitudes for mixed prospects where both gains and losses may be incurred. The next section presents the design of the tasks and incentives, which have been designed to minimize differences across domains and to minimize potential biases that could have led to ambiguity seeking in previous studies.

Section 3.3 describes basic properties of the data and section 3.4 presents results for the pure outcome domains (either gain or loss). Section 3.5 presents results for mixed prospects. Despite skewing our design towards ambiguity neutrality, we find clear evidence for the pattern predicted by Daniel Ellsberg in his 2011 commentary. Section 3.6 concludes with a short discussion of the implication of these findings for the modeling of ambiguity preferences.

3.2 Measurement of ambiguity attitudes

3.2.1 Prospects and domains

We elicit attitudes towards uncertain prospects defined on the outcome domain of gains or losses, and involving either low or moderate likelihoods. Participants make choices between ambiguous prospects and risky prospects. A risky prospect that pays €x with probability $p \in [0,1]$ and €y otherwise is denoted $x_p y$. An ambiguous prospect that pays €x if event E occurs and €y otherwise is denoted $x_E y$. Ambiguity attitudes are identified by comparing participants' preferences between risky prospect $x_p y$ and ambiguous prospect $x_E y$, where E is defined such that exchangeability of events implies that the subjective probability $B(E)$ equals p. In particular, the ambiguous prospects are implemented as bets on the color of a marble drawn from a bag with an unknown distribution of colors, but with the participant being indifferent between betting on either of these colors. Details on the procedure are given in section 3.2.3.

In the experiment we implement either moderate likelihood events with $p = 0.5$, or low likelihood events with $p = 0.1$. The outcomes x and y vary across experimental conditions. In treatments with pure outcome domains, x equals either €20 in the gain conditions, or $-$€20 in the loss conditions, while y always equals €0. In treatments with mixed prospects, x equals €10 and y equals $-$€10, or vice versa. All conditions are shown in the first three columns of Table 3.1, which defines the conditions in terms of the properties of the risky prospect employed in the comparison between risk and ambiguity.

Treatment[a]	Outcome domain	(Subjective) probability	# colors used to implement ambiguous events	Expected value of risky prospect	Predicted attitude[b]
$20_{.5}0$	Gain	$p = 0.5$	2	10	AA
$(-20)_{.5}0$	Loss	$p = 0.5$	2	-10	AS
$20_{.1}0$	Gain	$p = 0.1$	10	2	AS
$(-20)_{.1}0$	Loss	$p = 0.1$	10	-2	AA
$(-10)_{.5}10$	Mixed	$p = 0.5$	2	0	–
$(-10)_{.1}10$	Mixed	$p = 0.1$	10	8	–
$10_{.1}(-10)$	Mixed	$p = 0.1$	10	-8	–

Table 3.1: Treatments in a between-subjects design

Notes: a: risky prospect shown; b: based on pattern observed in the literature; AA=ambiguity aversion; AS=ambiguity seeking.

3.2.2 Measurement

We measure ambiguity attitudes in a between-subjects design with each subject participating in exactly one of the seven treatments shown in Table 3.1. Our preference elicitation procedures are designed to minimize (and control for) potential biases due to the measurement method itself. To this end, we measure attitudes in two stages. In stage 1, we elicit a direct binary choice between a risky prospect and its matched ambiguous prospect. While this choice provides the simplest test of ambiguity attitudes and involves basically no design issues, it only allows us to categorize subjects into ambiguity averters and ambiguity seekers (with neutrals included in both categories).

In stage 2, we then elicit *probability equivalents* q for the ambiguous prospect: we find the risky prospect $x_q y$ such that the participant is indifferent between the prospect $x_q y$ and $x_E y$. Note that if preferences are not ambiguity neutral, $B(E)$ implied by exchangeability need not be equal to q. Differences in q across subjects allow us to identify ambiguity attitudes more precisely. In particular, for gain prospects, a smaller q implies stronger ambiguity aversion as the decision maker is willing to accept a lower known chance of a gain in exchange for the unknown chance implied by E. For loss prospects a larger q implies ambiguity aversion as the decision maker is willing to accept a larger known chance of a loss in exchange for the unknown chance implied by E.

We elicit probability equivalents using a choice list consisting of nine binary choices, where choice i elicits the preference between the ambiguous prospect $x_E y$ and a risky prospect $x_{q_i} y$. The known probability q_i increases when going down the list of choices, while the outcomes x and y, and event E remain constant across choice items i. Table 3.2 shows the probabilities q_i used in the choice lists for moderate and for low likelihood events.

	Prob. q_i conditional on baseline likelihood p	
Decision item	$p = .5$	$p = .1$
1	0.25	0.01
2	0.30	0.04
3	0.35	0.07
4	0.40	**0.10**
5	0.45	0.13
6	**0.50**	0.16
7	0.55	0.19
8	0.60	0.22
9	0.65	0.25

Table 3.2: Choice lists for eliciting probability equivalents

Notes: Entries are known probabilities q_i; x, y, and E are constant across choices in this list and depend on the treatment (see Table 3.1).

When going through the choice list, a participant is presented with choices in which the known-risk prospect is initially very unattractive (for gains; opposite reasoning for losses), and subsequently becomes more attractive. If the initial risky prospect is less attractive than the ambiguous prospect and the ninth risky prospect is more attractive than the ambiguous prospect, there will be a probability q_i at which the decision maker is indifferent between the two prospects. We use the choice item at which the participant switches from a preference for ambiguous to a preference for risky to approximate her probability equivalent. Formally, for a subject who switches to risky after item $i \in \{0, 1, 2, \ldots, 9\}$ the probability equivalent q is given by

$$q = \begin{cases} q_1 - \frac{1}{2}(q_2 - q_1) & \text{if } i = 0; \\ \frac{1}{2}(q_{i+1} + q_i) & \text{if } i \in \{2, 3, \ldots, 8\}; \\ q_9 + \frac{1}{2}(q_9 - q_8) & \text{if } i = 9; \end{cases}$$

where $i = 0$ means that the participant chooses risky already in the first choice option, and $i = 9$ means that she never switches to risky. Hence, we take the midpoint between probabilities as an estimate, or, if necessary, extrapolate a probability equivalent at the boundaries of the choice list.[1]

While the stage 1 binary choices should not be prone to biases caused by the elicitation procedure, elicitation procedures that measure more detailed preferences are typically affected by design effects. The list of choices used to measure probability equivalents was designed to minimize design-driven biases, while accounting for the prior evidence and the structural differences between low and moderate likelihood prospects. Thus, we included the direct choice between $x_E y$ and $x_p y$ roughly in the middle of the choice list, skewing the list modestly towards the direction of ambiguity aversion for modest likelihood prospects and towards ambiguity seeking for low likelihood prospects. This design aims to reduce biases resulting form highly skewed choice lists for low likelihood prospects relative to modest likelihood prospects, while at the same time reducing the risk of having many subjects at the boundary of the choice lists by disregarding the previous evidence. In particular, because previous studies reported probability equivalents in the range of 0.3 for modest ($p = 0.5$) and 0.2 for low likelihood ($p = 0.1$) prospects (Trautmann and van de Kuilen, forthcoming), we wanted the choice list to cover these values away from the boundaries of the list. The inclusion of the basic comparison between $x_E y$ and $x_p y$ allows us to assess the robustness of the initial choice in the second occasion, and in particular when it is included in a full list of choices. The comparison of the option selected in

[1] In a third stage of the experiment we also elicited certainty equivalents for the risky prospect $x_p y$ as a measure of risk attitude. We observe the typical reflection effects between gains and losses, and overweighting of small and underweighting of large probabilities. In particular, we find significant risk aversion for gains with moderate likelihoods and for losses with low likelihoods; and significant risk seeking for the mirrored domains and likelihoods. We do not observe any significant correlations between ambiguity and risk attitudes, similar to findings reported by Cohen et al. (1987), Di Mauro and Maffioletti (2004), or Levy et al. (2010). We do not discuss this part of the study in the current paper.

stage 1 and stage 2 for this choice item provides us with a measure for the consistency of ambiguity attitudes across tasks (Binmore et al., 2012; Charness et al., 2013).

3.2.3 Experimental procedures

Following the classic Ellsberg thought experiments, risky and ambiguous prospects are implemented as opaque bags which are filled with exactly 100 chips of different colors. In the moderate likelihood treatments ($p = 0.5$), bags contain at most two colors; in the low likelihood treatments ($p = 0.1$), bags contain at most ten different colors. At the beginning of the experiment and before any instructions for stage 1 are handed out, we ask each participant to choose a personal "decision color" from the list of possible colors, which will remain fixed throughout the experiment. At that point, participants are not aware of any experimental details, thus their beliefs about events in the Ellsberg tasks cannot affect their color choice. Participants are informed that the selected color will be relevant for determining their payoffs.[2]

For the ambiguous prospect, an opaque bag has already been filled with 100 colored chips at the time when subjects enter the laboratory. The distribution of colors is unknown to subjects as well as to the experimenters, though we do allow participants to inspect this bag as soon as the experiment is over.[3] For each risky prospect $x_{q_i}y$ we prepare a bag that contains exactly $q_i \times 100$ red chips and $(1 - q_i) \times 100$ chips of the remaining colors. Thus, the prospect pays x if a red chip is drawn from the respective bag, and y if another chip is drawn. In contrast, the ambiguous prospect pays x if a chip of the participant's personal decision color is drawn from the ambiguous bag, and y otherwise. By letting subjects choose their personal decision color it is obviously impossible for the experimenter to trick subjects or bias the distribution of colors in the ambiguous bag (Charness et al., 2013). Additionally, in the instructions of each part, we remind subjects that they have chosen their decision color themselves. Further, to facilitate an understanding of risky prospects, the corresponding distributions of chips are placed on a table in the lab room, visible to participants during the experiment (see Figure C.1 in appendix C.2).

At the beginning of the experiment subjects receive an endowment of €20. This endowment is identical across treatments to avoid any effects from variations in initial wealth. Additionally, one stage of the experiment is randomly selected to be payoff relevant for all participants in a particular session at the end of the experiment. Within a choice list, one decision item is randomly selected to be paid out. This selection is randomized at

[2] In the low likelihood treatments popular colors such as red, blue and green are chosen most often; apart from that color choices are distributed quite evenly. Details are provided in Figure C.3 and Table C.4 in appendix C.4.

[3] Before the experiment, a student assistant blindly drew 100 chips from an opaque bag filled with in total roughly 1000 chips of all respective colors. From the instructions subjects only learn that a student assistant drew 100 chips from a bag that contains considerably more than 100 chips.

the individual level. Because the experiment was computerized (using zTree; Fischbacher, 2007), we aimed to ensure credibility regarding design and procedure (i) by implementing prospects in a concrete and verifiable way; (ii) by allowing subjects to define the ambiguous prospect through their individual decision color; and (iii) by randomly selecting one participant as an assistant at the end of the experiment. This person is in charge of randomly selecting the payoff-relevant stage, of filling the risky bags, and of finally drawing one chip out of each bag. Each step is performed in front of the other participants, and outcomes are entered on the assistant's computer screen.[4] Any earnings are added to the initial endowment.

While subjects might earn up to €40 in the gain treatments, they might end up with zero income in the loss treatments. In order to smooth expected income across treatments we added an effort task based on Raven's progressive matrices (Raven et al., 1998) at the end of the experiment, in which subjects can earn an additional amount, which is negative in the gain, and positive in the loss treatments.[5] We do not provide any feedback on the outcome of preceding stages during the experiment and instructions are not handed out until the previous task had been finished. Sample instructions for the $20_{.5}0$ treatment are provided in appendix C.2.

3.3 Data description

In total 501 subjects participated in 21 experimental sessions, with three sessions for each treatment condition. 58% of participants were female, the average age was 24.5 years, and 21% were economics or business students. The experiment lasted roughly one hour and participants earned on average €22.30 (approx. $29.30 at the time the experiment was conducted).

We did not enforce single switching points in the choice list of stage 2, and as often observed, some subjects switched more than once between ambiguous and risky prospects when moving down the list. If the person chooses ambiguity in the first and risk in the last choice item (for gains and mixed prospects with $x > 0$; vice versa for losses and mixed prospects with $x < 0$), we deal with these violations of monotonicity by calculating the

[4] First, the assistant drew one ball out of an opaque bag containing three numbered balls, to determine the payoff relevant stage. If stage 1 was selected, the assistant filled one opaque bag with the distribution of chips defining the risky prospect x_py. If stage 2 was selected, the assistant filled nine opaque bags, one for each decision item of the respective choice list. In total, ten different bags might be relevant, one ambiguous and nine risky ones. The assistant then drew one chip from each bag.

[5] Subjects have to solve ten effort tasks, which presented them with 3×3 matrices of graphical figures, with one cell left blank. Within 45 seconds, the subject had to select the correct figure out of six different options, filling in the blank to complete the logical sequence of the matrix. In the gain treatments subjects incur a loss for every incorrect answer; in the loss treatments subjects earn a positive amount for every correct answer; in the mixed treatments subjects either face positive, negative or no incentives, which depends on whether x_py yields a negative, positive or zero expected value, respectively.

probability equivalent as the midpoint over the range defined by her first and by her last switching point. If the person does not start from ambiguous and eventually switch to and remain at risky, such a calculation is impossible and we drop the respective observation from the sample of stage 2 choices.[6] This leaves us with 289 stage 1 choices and 280 valid stage 2 probability equivalents in the pure outcome treatments, and 212 stage 1 choices and 204 valid stage 2 probability equivalents in the mixed outcome treatments.

We classify subjects as ambiguity averse or ambiguity seeking as follows. In the stage 1 direct choice, a subject is classified as ambiguity averse (seeking) if she prefers the risky (ambiguous) prospect. In stage 2, for gain treatments (and in mixed with $x > 0$) a subject is classified as ambiguity averse (seeking) if the probability equivalent q is smaller than p (is larger than p). Analogously, in the loss treatments ($x < 0$) a subject is classified as ambiguity averse (seeking) if the probability equivalent q is larger than p (is smaller than p).

While binary choices in stage 1 do not allow identifying indifference between the ambiguous and risky prospect, ambiguity neutrality can be detected on the basis of stage 2 probability equivalents. Ambiguity neutrality implies indifference between risky and ambiguous prospect if and only if $q = p$ (where $p = 0.5$ or $p = 0.1$). Thus, ambiguity neutral subject will either switch in the decision item in which the known probability q_i is equal to p, or in the subsequent one.[7] In the following we first give results without specific consideration of ambiguity neutrality, and then discuss its extent and its impact on the elicited pattern of attitudes in more detail. We subsequently analyze the consistency of ambiguity attitudes across stages 1 and 2.

3.4 Ambiguity attitudes for pure outcome domains

3.4.1 Basic results

The left panel of Table 3.3 summarizes the results for the pure-domain treatments based on the whole sample.[8] The table indicates the direction of the preference in each condition and stage (AA ambiguity aversion; AS ambiguity seeking; insignificant effects in parentheses). We replicate the typical finding of ambiguity aversion in the classic Ellsberg setting

[6] Overall the rate of inconsistencies was low: 5.8% of all choice lists involved inconsistencies (5.2% in pure domains and 6.6% in mixed domains), and only 3.4% of all choice lists had to be dropped from the sample (3.1% in pure domains and 3.8% in mixed domains). These rates are well within the bounds typically observed in empirical studies. Results do not change substantially if we instead drop all choice lists involving multiple switching, or if we exclude stage 1 observations for those people who were inconsistent in stage 2 (shown in Table C.5 and C.6 in appendix C.4).

[7] That is, in the comparison between q_i and p they may choose either option since they are indifferent. Details and a graphical representation of neutrality are provided in appendix C.3.

[8] All tests reported in the paper are two-sided tests. We acknowledge that the predicted fourfold pattern of attitudes could generate one-sided hypotheses. The interpretation of our results would not change if one-sided tests were used accordingly.

(i.e., $20_{.5}0$): a minority of 38.9% prefers the ambiguous prospect in stage 1 (binomial test, p=0.076), and probability equivalents are modestly, but significantly smaller than 0.5.

	Whole sample			*Distinct from neutrality* [b]		
Treatment	# obs. Stage 1 (Stage 2)	Stage 1: ambiguous choices (%)	Stage 2: probability equivalent[a]	# obs. Stage 1 (Stage 2)	Stage 1: ambiguous choices (%)	Stage 2: probability equivalent[a]
$20_{.5}0$	72 (70)	38.9 AA*	.48 AA***	26 (26)	15.4 AA***	.43 AA***
$(-20)_{.5}0$	73 (71)	47.9 (AA)	.53 (AA)	17 (16)	64.7 (AS)	.43 AS**
$20_{.1}0$	71 (67)	57.7 (AS)	.12 AS***	21 (20)	71.4 AS*	.15 AS***
$(-20)_{.1}0$	73 (72)	63.0 AS**	.09 (AS)	25 (24)	28.0 AA**	.15 AA*

Table 3.3: Ambiguity attitudes for pure outcome domains

Notes: a: median; b: classification as described in section 3.3; direction of effect: AA=ambiguity averse; AS=ambiguity seeking; *,**,*** denote significance at the 10%, 5%, and 1% level; Part 1: two-sided binomial test against p=0.5; Part 2: two-sided t-test against probability equivalent=0.5/0.1.

In contrast, for the other outcome domains we do not find any ambiguity aversion. Behavior in the moderate likelihood loss domain is indistinguishable from ambiguity neutrality (defined here as 50% of subjects choosing either prospect in stage 1, and $q = 0.5$ in stage 2). For the two low likelihood prospects we observe ambiguity seeking, although the preference is only statistically significant in stage 2 for $20_{.1}0$ and in stage 1 for $(-20)_{.1}0$. That is, considering all domains, there is little evidence for universal ambiguity aversion: pooling all stage 1 choices from pure domain treatments does not indicate any ambiguity attitude (binomial test, p=0.556); classifying subjects as ambiguity averse or ambiguity seeking according to their probability equivalent, we again no significant tendency towards ambiguity aversion (binomial test, p=0.106). However, we also observe that the pattern of attitudes shown in the left panel of Table 3.3 is not consistent with the fourfold pattern identified in the literature (shown in Table 3.1, column 6), because the pattern would predict ambiguity aversion for the $(-20)_{.1}0$ loss treatment. In contrast, significant ambiguity seeking is found in this domain.

3.4.2 Accounting for ambiguity neutrality

A significant share of the participants exhibits ambiguity neutrality as defined in section 3.3. Columns 1 to 4 in Table 3.4 show that the share of ambiguity neutral subjects in the pure outcome domains falls in the range 62.9% up to 77.5% (related histograms of probability equivalents are provided in Figure C.2 in appendix C.3). These are large percentages. With respect to the basic Ellsberg domain $20_{.5}0$ that suggests that while many subjects have a tendency to make ambiguity averse choices, the strength of these preferences might in fact be modest.

As described in section 3.3, ambiguity neutral subjects may make choices in different

Treatment						
$20_{.5}0$	$(-20)_{.5}0$	$20_{.1}0$	$(-20)_{.1}0$	$(-10)_{.5}10$	$(-10)_{.1}10$	$10_{.1}(-10)$
62.9%	77.5%	70.2%	66.7%	82.4%	67.2%	81.2%

Table 3.4: Ambiguity neutral subjects by treatment

Notes: Entries report percentages of subjects with probability equivalent in the interval [0.475, 0.525] in treatments with moderate likelihoods, and in [0.085, 0.115] in treatments with low likelihoods. See section 3.3 for details.

ways that are both consistent with their preferences but lead to different categorizations in terms of ambiguity attitude. This might affect the observed patterns of attitudes in the four domains of interest. The right panel of Table 3.3 shows results including only those subjects who are not identified as ambiguity neutral. Although the sample sizes are strongly reduced, a highly significant and consistent (across stages) pattern emerges: strong ambiguity aversion for moderate likelihood gains and low likelihood losses, and strong ambiguity seeking for moderate likelihood losses and low likelihood gains. That is, participants who are not ambiguity neutral strongly reveal the fourfold pattern of ambiguity attitudes suggested by the review of the literature. Moreover, because the pattern obtains for both binary choices and probability equivalents, it seems unlikely that it is driven by choice list design effects that might have influenced previous results. However, we observe that the deviations from neutrality in the stage 2 task are more modest than some reports in the literature, suggesting that our design reduced biases in the elicitation methods.[9]

3.4.3 Consistency across elicitation tasks

The consistency of ambiguity attitudes over repeated choices in the same experiment has been questioned in some studies (Binmore et al., 2012; Dürsch et al., 2013; Stahl, 2014). The inclusion of the stage 1 choice between $x_E y$ and $x_p y$ in the stage 2 choice lists allows us to examine consistency on the individual level. Table 3.5 shows results.

Overall consistency across stages is high, with the standard Ellsberg task $20_{.5}0$ being at the lower end of the range of consistency rates with about 72% of participants choosing

[9]We noted in section 3.2.2 that choice lists in stage 2 are modestly skewed (such that the choice between $x_E y$ and $x_p y$ is not centered within a list; compare Table 3.2). If subjects, however, tend to switch towards the center of a list, stage 2 choices might be biased. More precisely, probability equivalents would be biased downwards in moderate likelihood treatments, overestimating ambiguity aversion in the gain domain and ambiguity seeking in the loss domain, and biased upwards in low likelihood treatments, overestimating ambiguity seeking in the gain domain and ambiguity aversion in the loss domain. Especially ambiguity neutrals, not identified in stage 1, might be biased towards non-neutral attitudes in stage 2. Yet, stage 1 choices are unaffected by any such bias and, thus, can be used as a control: if the design of stage 2 choice lists overestimates the reported fourfold pattern of attitudes, we would expect to observe different results (over the whole sample) in stage 1 compared to stage 2. This is clearly not the case given the high consistency rates, as discussed next in section 3.4.3, across elicitation tasks, in each cluster.

	Treatment						
	$20_{.5}0$	$(-20)_{.5}0$	$20_{.1}0$	$(-20)_{.1}0$	$(-10)_{.5}10$	$(-10)_{.1}10$	$10_{.1}(-10)$
Including ambiguity neutrals	72.9%*	88.7%*	82.1%*	79.2%*	72.1%*	74.6%*	78.3%*
Excluding ambiguity neutrals	88.5%*	93.8%*	80.0%*	75.0%*	75.0%	63.6%	92.3%*

Table 3.5: Consistency of ambiguity attitude

Notes: Entries report percentages of subjects who make consistent choice in the identical choice item in stage 1 and stage 2 of the experiment. * indicates that the percentage is larger at the 5% significance level than expected under random choices (50% consistency).

consistently. Ambiguity neutrals may choose differently in both stages simply because they are indifferent between the risky and the ambiguous prospect. However, excluding ambiguity neutrals we find similar rates of consistency, with rates increasing in some treatments and decreasing in others.[10]

3.5 Ambiguity attitudes in the mixed domain

Under the benchmark assumption of universal ambiguity aversion, the mixed domain received little attention in ambiguity research. However, with the fourfold pattern emerging from empirical the literature, the mixed domain becomes an important testing ground for models of ambiguity attitude that can account for domain specificity (Tversky and Kahneman, 1992; Klibanoff et al., 2005; Abdellaoui et al., 2011). It also relates more directly to the type of prospects experienced by decision makers in financial markets, medical decisions, or legal decisions, for example.

We consider three mixed prospects: a symmetric prospect with an equal chance to win or lose €10; an advantageous prospect $(-10)_{.1}10$ with a low likelihood loss and a high likelihood gain event; and a disadvantageous prospect $10_{.1}(-10)$ with a low likelihood gain and a high likelihood loss event. As Table 3.4 shows, there are many ambiguity neutral subjects also for the mixed outcome domain, and we therefore report results for both the whole sample, and the sample restricted to subjects who are not classified as ambiguity neutral. Results are shown in Table 3.6.

Three insights obtain from Table 3.6. First, there is little evidence of (universal) ambiguity aversion for mixed prospects. Second, there is less consistency of the pattern of attitudes across stages than for the pure domains: neither the full nor for the reduced sample reveal a consistent pattern for all three prospects. Third, the only robust finding concerns the ambiguity seeking observed for the prospect $10_{.1}(-10)$.

[10] Additional arguments why results reported in section 3.4.2 should not be confounded by inconsistent preferences are provided in appendix C.5.

Treatment	# obs. Stage 1 (Stage 2)	Stage 1: ambiguous choices (%)	Stage 2: probability equivalent[a]	# obs. Stage 1 (Stage 2)	Stage 1: ambiguous choices (%)	Stage 2: probability equivalent[a]
	Whole sample			*Distinct from neutrality* [b]		
$(-10)_{.5}10$	73 (68)	34.2 AA***	.53 (AA)	16 (12)	37.5 (AA)	.43 (AS)
$(-10)_{.1}10$	69 (67)	58.0 (AS)	.12 AA***	23 (22)	56.5 (AS)	.15 AA**
$10_{.1}(-10)$	70 (69)	67.1 AS***	.12 AS**	13 (13)	84.6 AS**	.15 AS**

Table 3.6: Ambiguity attitudes for the mixed outcome domain

Notes: a: median; b: classification as described in section 3.3; direction of effect: AA=ambiguity averse; AS=ambiguity seeking; *,**,*** denote significance at the 10%, 5%, and 1% level; Part 1: two-sided binomial test against p=0.5; Part 2: two-sided t-test against probability equivalent=0.5/0.1.

3.6 Conclusion

In this paper we elicit the ambiguity attitudes of more than 500 participants in four pure and three mixed outcome domains, at different levels of likelihood. Our measurement methods were designed to minimize biases caused by the elicitation method. In particular, we aimed to provide a conservative test of the ambiguity seeking tendencies observed in the previous literature. We also minimized design heterogeneity across domains: heterogeneity in terms of payoffs (real vs. hypothetical; endowment vs. no endowment), in terms of the presentation of the ambiguous prospects (Ellsberg urns, second order probabilities), and in terms of the degree to which participants' beliefs about the distribution of colors in the Ellsberg urns are controlled for.

We find no evidence for universal ambiguity aversion as it is assumed by basically all theoretical applications in various subfields of economics and finance today. A large share of the participants in our experiments can be categorized as ambiguity neutral. For those subjects who reveal clear deviation from neutrality, a fourfold pattern of ambiguity attitudes strongly emerges from the data: ambiguity aversion is found for modest likelihood gain (as in the classic Ellsberg paradox) and low likelihood loss prospects. Ambiguity seeking is found for low likelihood gain prospects and modest likelihood loss prospects. In all domains, a large group of subjects (a majority in fact) is close to ambiguity neutrality. This points to the importance of heterogeneity in ambiguity attitude for market outcomes (demonstrated in Bossaerts et al., 2010), across all domains of interest.

We find high but not perfect rates of consistency across stage 1 and stage 2 choices in our experiment. These effects suggest that choice list format does affect respondents' decisions. Consequently, estimates of the absolute degree(s) of ambiguity aversion observed some specific measurement should be interpreted with care: these levels will be affected by idiosyncratic design effects. In contrast, as long as general patterns and comparative results are central, more robustness can be expected.

Finally, with virtually all relevant problems in economics and finance relating to the mixed outcome domain, more empirical investigation of this domain seems warranted. We provide a first measurement of ambiguity attitudes for mixed prospects. As for the pure domains, we find little evidence of universal ambiguity aversion. Results seem also less robust, however, than those for the pure domains. The challenge for descriptive theoretical work on ambiguity will be to provide a model that can account for both the pattern observed for the pure domains, as well as for the attitudes revealed in the mixed domain.

Chapter 4

Social Anchor Effects in Decision-making under Ambiguity

4.1 Introduction

Peer effects are documented in a variety of economic decisions, such as stock market participation (Hong et al., 2004), insurance purchases (Cai et al., forthcoming), or investment choices (Bursztyn et al., 2014). Many of these environments involve ambiguity rather than risk, i.e., are characterized by distributions over outcomes which are likely to be unknown (Keynes, 1921; Knight, 1921). "*We might say, for example, that we do not know, when we go on a railway journey, the probability of death in a railway accident, unless we are told the statistics of accidents in former years; or that we do not know our chances in a lottery, unless we are told the number of the tickets.*" (Keynes, 1921, p. 31). Similarly, we might argue that investors usually do not know expected returns in exact numbers, nor do insurance customers perfectly assess the probability for a loss. Likewise, when choosing pension plans contributors are not able to predict interest rates curves in fixed-income markets. Not surprisingly, people often appear to be averse against ambiguous compared to risky situations. Intuitively, individuals may not feel confident in assessing true probabilities, especially if outcomes might entail detrimental consequences. Such ambiguity aversion might inhibit individuals from taking up costless genetic tests (Hoy et al., 2014) and explain the stock market participation puzzle (Dow and Werlang, 1992; Easley and O'Hara, 2009), among other examples. At the same time, ambiguity aversion has been found to correlate with irrational appraisals of ambiguous situations, such as over-pessimistic beliefs about probabilities or a perceived lack of information (Keren and Gerritsen, 1999). These behavioral biases do not seem consistent with normative models of individual decision-making under uncertainty. Furthermore, in contrast to risk aversion which might, for example, protect individuals from irrecoverable economic shocks by buying insurance, ambiguity aversion might induce truly suboptimal decisions in terms of departures from subjective expected utility theory (Savage, 1954) and probabilistic sophistication (Machina and Schmeidler, 1992). The conjecture that learning whether others also fear (or even seek) ambiguous events might enforce such irrational behavior appears straightforward. However, learning that others treat risk and ambiguity in a fairly indifferent manner might also convey a doctrine of ambiguity neutrality. In this paper I study individual ambiguity attitudes and examine how these attitudes are affected by feedback about others' choices under ambiguity.

Models on rational learning under ambiguity predict that Bayesian decision makers, who update their beliefs about ambiguous probabilities, should asymptotically converge towards ambiguity neutral preferences (see, e.g., Epstein and Schneider, 2007). In addition to receiving signals about outcomes from ambiguous lotteries, learning about others' decisions might influence subjective probabilities of a probabilistically sophisticated decision maker, or impact his[1] confidence in the ambiguous environment.[2] Some experimental evidence indeed shows that social interaction might induce a shift in attitudes towards ambiguity neutral preferences (Keck et al., 2011; Charness et al., 2013). In contrast, studies from social psychology suggest that choices of others generally provide an anchor to which decision makers wish to conform (e.g., Festinger, 1954; Cialdini and Trost, 1998; Cialdini and Goldstein, 2004). In the economic literature, models of distributional social preferences, such as the concept of inequity aversion by Fehr and Schmidt (1999)[3], have rather been used to explain peer effects in terms of imitative behavior (for example, in consumption choices or asset pricing, see Galí, 1994; Gebhardt, 2004, 2011, respectively). Either approach predicts changes towards the peer's ambiguity attitude, suggesting that prevailing attitudes might even be corroborated through feedback about others.

This paper provides additional evidence on shifts in ambiguity attitudes contingent on learning the choices of peers. Following the standard two-color Ellsberg setting (Ellsberg, 1961) I elicit probability equivalents in a laboratory experiment, which reflect indifferent preferences between a risky and an ambiguous prospect. Ambiguity attitudes are measured individually in a first part, and again in a second part, with respect to the same prospects and using the same task. In a between-subjects design, I exogenously vary whether lotteries are defined over gains or losses, and whether subjects are shown previous choices of another participant – additionally to their own – when making their choices a second time. I refer to this as providing subjects with a *social anchor* of a *peer*.[4] If a

[1] I refer to a decision maker as "he" throughout this paper.

[2] Epstein and Schneider (2007) introduce learning in the maximin framework of Gilboa and Schmeidler (1989); in particular they allow a Bayesian decision maker to adjust his confidence as well as his beliefs. Learning under ambiguity is also addressed in models by Huber (1973), Walley (1991), and Marinacci (2002). Models that incorporate inter-temporal settings, such as Epstein and Wang (1994) and Epstein and Schneider (2003) who build on the maximin model, establish a basis for these contributions.

[3] For a survey on models of distributional preferences see Camerer (2003) and Fehr and Schmidt (2006).

[4] The term *anchor* is often used to describe "a stimulus or a message that is clearly designated as

social anchor is available, subjects may have two incentives to take the peer's decisions into account: first, the rational information inherited in the peer's decisions, providing an additional anchor for evaluating the choice situation; second, having social preferences with respect to the peer's outcome. By comparing treatments with and without social anchor, changes in ambiguity attitudes which may, e.g., be due to reconsideration or increased familiarity with the experimental task, can be distinguished from changes that result from learning others' choices.

Generally, the data suggests that ambiguity attitudes constitute a stable part of preferences: 90% of subjects exhibit the same attitude (ambiguity aversion or ambiguity seeking) in the first and in the second part, independent of having a social anchor available or not. However, the intensity of these attitudes does by far not appear to be as robust, and varies between Part 1 and Part 2 in 50% of all cases. In the analysis I, hence, particularly focus on the frequency with which subjects change their probability equivalents, and on the direction of shifts in ambiguity attitudes, conditional on such a change.

My key finding is that individual dynamics and peer effects in ambiguity attitudes considerably differ in the loss compared to the gain domain. In the domain of gains, the individual's ambiguity attitude does not have any significant effects on the likelihood to change or on the direction of a change, neither in the absence nor in the presence of a social anchor. However, learning to be more ambiguity averse than a peer significantly increases the likelihood to change, relative to the individual condition. That is, the *relative* ambiguity attitude, i.e., the ambiguity attitude compared to the peer's, matters. Further, conditional on a change in probability equivalents, decision makers tend to imitate their peer's attitude, towards ambiguity aversion, seeking, or neutrality. Ultimately, the provision of a social anchor in the gain treatments predominantly induces subjects who perceive themselves to be comparably ambiguity averse to shift towards neutrality.

In the domain of losses, in contrast, individual attitudes significantly matter in the individual treatment, such that ambiguity seeking subjects are significantly more likely to change compared to ambiguity averse ones. However, this relationship fades out when a social anchor becomes available. In the social anchor treatments it is again the relative ambiguity attitude that matters, but in a different way compared to the gain domain: if peers learn to exhibit exactly the same attitudes, their likelihood to change is significantly reduced, relative to the individual condition, which I label a *reassurance effect*. Again in contrast to the gain domain, conditional on a change in probability equivalents, I observe significant shifts from ambiguity seeking towards neutrality, with and without social an-

irrelevant and uninformative" (Kahneman, 1992, p. 308). I slightly deviate from this definition and use the term to describe additional information which should be irrelevant for a rational and selfish decision maker. In this setting, choices of others might, however, be relevant in individual decision-making if individuals exhibit distributional preferences or perceive others to be more competent with respect to the task.

chor. Overall, these findings suggest that ambiguity seeking might not be particularly robust in such settings (in line with experimental evidence as discussed below).

Moreover, I find that cognitive ability significantly and positively correlates to shifts towards neutrality in the gain domain, while any relationship is of negligible relevance in the loss domain. This suggests that ambiguity aversion in the standard Ellsberg setting might be driven by bounded rationality, while ambiguity seeking might rather be of instinctive nature.

Overall, my data suggests three main conclusions. The intensity of ambiguity attitudes is likely to fluctuate, even if no social anchor is available; nevertheless, peers seem to be important for decision-making under ambiguity; and being provided with a social anchor seems to affect individual attitudes differently in the domain of gains compared to losses.

In the economics literature social interaction in decision-making under ambiguity has been examined early on. In a seminal paper Curley et al. (1986) find evidence for the "Other-evaluation hypothesis". That is, if individuals anticipate negative evaluation by others, they tend to make choices which are perceived to be most justifiable to others; and often, the choice most easily justified seems to be the ambiguity averse.[5] Intuitively, a bad outcome might be indisputably deluded to bad luck if it results from a risky prospect, which would not be as easy if it results from an ambiguous prospect. That the fear of negative ex-post evaluation by others is likely to increase ambiguity aversion is also confirmed by Trautmann et al. (2008) and Muthukrishnan et al. (2009). Basically, these studies already suggest how sensitive ambiguity attitudes might be with respect to changes in the social environment.

Two other studies are particularly related to the present paper. Keck et al. (2011) and Charness et al. (2013) (in the following referred to by KDB and CKL, respectively) compare ambiguity attitudes in the domain of gains, elicited in isolation and elicited after face-to-face consultation with peers. Both studies find that social interaction causes a shift in ambiguity attitudes towards neutrality. In CKL, this shift predominantly stems from ambiguity seeking individuals and those who exhibit incoherent attitudes. KDB, on the other hand, report that the shift towards neutrality is caused by a reduction in both ambiguity aversion and ambiguity seeking.[6] My results are consistent with their findings

[5]For example, Heath and Tversky (1991), Fox and Tversky (1998), and Fox and Weber (2002) show that individuals become more ambiguity averse if they make choices in the presence of others who they perceive to be more competent than themselves. Following the terminology of Watson and Friend (1969) the Other-evaluation hypothesis is also referred to as the "fear of negative evaluation", and is also addressed in Knight (1921), Fellner (1961), Ellsberg (1963), Roberts (1963), Toda and Shuford (1965) and Gärdenfors (1979).

[6]CKL also introduce incentives to persuade peers, which significantly increase peer effects but leaves the tendency towards neutrality at the expense of ambiguity seeking and incoherent attitudes unchanged. KDB additionally elicit decisions made in groups, and compare "shared consequences" to "individual consequences" in outcomes. Group decisions also show a tendency towards ambiguity neutrality, which is largely independent of payoff complementarities. In a different paper on group decision-making, Keller et al. (2007) find that groups exhibit risk and ambiguity aversion and are likely to exhibit a cautious shift

as they show that subjects take information about their peer's attitude into account when making individual choices. I observe significant shifts in ambiguity attitudes, although not exclusively towards ambiguity neutrality, but predominantly so if losses are involved. At this point, it is important to note that having a social anchor available compared to interacting with others face-to-face might have very different effects. In particular, following the arguments of CKL and KDB, face-to-face interaction might be even more powerful in enforcing ambiguity neutrality as a persuasive argument (Burnstein and Vinokur, 1977). This paper complements both studies in several respects. First of all, to cleanly isolate the effect of peers, I implement a baseline treatment which allows me to control for changes in ambiguity attitudes due to repeated decision-making. Thus, I control for the possibility that individuals might move towards ambiguity neutral preferences if they are only given the opportunity to reconsider their choices.[7] In this way, I am able to show that significant shifts from ambiguity seeking to ambiguity neutrality occur even if a social anchor is not available. I also abstain from allowing face-to-face interaction between participants since these are not under the control of the experimenter – although direct consultation might induce stronger peer effects.[8] Further, in my experiment subjects do not experience any outcomes between successive decisions (as in the experiment of CKL), as this may cause income effects or changes in ambiguity attitudes through the signal they received. Moreover, I particularly focus on the standard 50/50 Ellsberg setting which has been of most interest in the experimental literature and, hence, in which experimental achievements on individual ambiguity attitudes might be most reliable. Although empirical applications in finance or insurance might often relate to asymmetric distributions, I constitute a foundation of results in a well-studied setting that can be easily extended to, for example, low probability events or ambiguity over mixed domains. Moreover, the elicitation method allows me to cleanly distinguish between ambiguity aversion and ambiguity seeking, and provides a measure for the intensity of these attitudes.[9] In this respect, eliciting probability equivalents has been shown to identify ambiguity aversion, neutrality, and seeking in various domains (see, e.g., chapter 3 of this dissertation). I restrict from using di-

when ambiguity is introduced.

[7]This has been neglected in both studies above. KDB implement a control treatment, but only to validate that group choices are closer to ambiguity neutrality compared to individual choices.

[8]Keck and co-authors even state that "subjects were particularly instructed to take into account the opinion and attitudes of group members when making their subsequent individual decisions" (Keck et al., 2011, p. 21).

[9]CKL use a three-color Ellsberg design and employ a choice list including direct choices between risk and ambiguity. They report a substantial fraction of ambiguity incoherent attitudes, which might be partially due to the fact that the likelihood of the known and ambiguous colors change both at the same time across decision items. KDB elicit certainty equivalents for risky and ambiguous gambles, using multiple choice lists for different likelihoods and degrees of ambiguity. This implies that subjects fill out a considerable number of choice lists with a considerable number of decision items. When receiving feedback about others subjects may learn about their peer's inconsistencies, and, moreover, disentangling effects over time (e.g., individual decisions might be biased towards the end of the task), order effects and peer effects, is statistically demanding.

rect choice methods which might stimulate ambiguity aversion (Trautmann et al., 2011), and which would not permit to identify changes in attitudes due to indifference between ambiguity and risk. In the experiment I also minimize any suspicion of subjects that ambiguous lotteries might be stacked against their favors. I do so by letting participants choose their individual decision color which indirectly defines their ambiguous prospect.

Notably, all of the studies mentioned above involve prospects in the gain domain only. But given that numerous experiments documented differences in ambiguity attitudes with respect to the outcome domain, it seems obvious to extend this literature by studying behavior in the gain and loss domain. More precisely, while there is broad consensus about the persistence of ambiguity aversion in settings that involve prospects over gains of moderate likelihoods, evidence on preferences over ambiguous prospects which involve low likelihood events and/or losses are less clear-cut (see, e.g., Camerer and Weber, 1992). If outcomes refer to losses some studies report ambiguity seeking, while others document ambiguity neutrality as the predominant attitude.[10] I contribute to the literature by showing that individual and social dynamics as well as cognitive ability might shape ambiguity attitudes in very different ways, depending on the specific outcome frame.

Examining the role of peers in choice under ambiguity and understanding when and why consultation with others or receiving professional advice might be beneficial, has important implications, e.g., for decision-making in finance. In this respect, field studies highlight the role of observing other people's behavior. For example, Bursztyn et al. (2014) report that investment rates substantially increase if private investors can learn from their peer's investment choice, and if relative payoff concerns might additionally play a role. Cai et al. (forthcoming) examine insurance demand in rural China and observe significant spillover effects of insurance knowledge and experience, ultimately affecting insurance demand. Yet, the particular sources of imitative behavior cannot be completely disclosed, e.g., it is not clear whether such peer effects are due to changes in ambiguity attitudes or due to other factors, such as changes in the willingness to take risk. While disentangling the role of peers is difficult empirically (Manski, 1993), laboratory experiments provide an important complement to field studies to identify the underlying sources of peer effects, by cleanly controlling exogenous variations in the decision-making and social environment. However, experimental studies on social interaction in *ambiguous* choice situations are still rare, in contrast to studies on peer effects in decision-making under *risk*, which have

[10] Ambiguity seeking was reported by Casey and Scholz (1991); Di Mauro and Maffioletti (1996); Ho et al. (2002); Abdellaoui et al. (2005); Chakravarty and Roy (2009); Baillon and Bleichrodt (2013); Kothiyal et al. (2014); ambiguity neutrality was found by Einhorn and Hogarth (1986); Cohen et al. (1987); Mangelsdorff and Weber (1994); Eisenberger and Weber (1995); Du and Budescu (2005); De Lara Resende and Wu (2010); Trautmann and Wakker (2012); Tymula et al. (2012). Some studies also report a fourfold pattern, documenting ambiguity aversion for prospects over gains of moderate likelihoods and losses of low likelihoods, but ambiguity seeking for prospects over gains of low likelihoods and losses of high likelihoods (Hogarth and Kunreuther, 1985; Kahn and Sarin, 1988; Hogarth and Kunreuther, 1989; Viscusi and Chesson, 1999; Di Mauro and Maffioletti, 2004; Vieider et al., 2012).

received great attention lately (for a recent survey see Trautmann and Vieider, 2011). This paper takes one step to bridge the gap between field studies on peer effects in decisions under ambiguity and laboratory studies on peer effects in risk taking. Whether social anchors effects work differently depending on initial individual ambiguity attitudes is just one question which can be tested in the lab.

This paper proceeds as follows. Section 4.2 describes the experimental design, results are presented in section 4.3. Section 4.4 provides a discussion of findings and concludes.

4.2 Experimental design

I conduct a laboratory experiment and examine individual ambiguity attitudes in the absence and in the presence of feedback about others' choices under ambiguity. By eliciting individual choices twice, in Part 1 and Part 2 of the experiment, and comparing changes in choices across treatments, I control for the variability of ambiguity attitudes, e.g., for any effects which occur through familiarity of subjects with the particular elicitation task. Part 1 and Part 2 are identical to a large extend: ambiguity attitudes are elicited with respect to the same ambiguous prospect, using the same task which is described in section 4.2.1. The experiment is then based on a 2×2 between-subjects design with respect to two dimensions, as outlined in detail in section 4.2.2: first, the outcome domain of lotteries varies between gains and losses. Second, in Part 2, subjects either only face their own choices made in Part 1, or they are additionally provided with a *social anchor*, that is, learn the choice profile of another participant.

After Part 2, subjects face an intelligence test which is independent of any treatment variation. It provides a measure for subjects' cognitive ability and allows to smooth income from Part 1 and 2 which might vary considerably between gain and loss treatments.[11] Experimental procedure are summarized in section 4.2.3.

4.2.1 Elicitation task

Ambiguity attitudes are measured in terms of probability equivalents with respect to an ambiguous prospect which is described by an urn, labeled A in the following.

Urn A contains in total 100 chips of two different colors (red and blue), reflecting the standard Ellsberg setting of moderate likelihoods with an ambiguity neutral probability $p = 0.5$. The distribution of red and blue chips is *unknown* to both subjects and the experimenter. To ensure credibility and avoid any suspicion that urn A might be biased against subjects, every participant is asked to select his individual decision color (red or blue) in the very beginning of the experiment, i.e., before the instructions for Part 1 are

[11] In this task subjects had to solve ten questions on Raven's progressive matrices (Raven et al., 1998), each incentivized by a piece rate. Details can be received from the author.

handed out. A subject's decision color indirectly defines the ambiguous prospect that he can choose to play in the experiment: if a chip is drawn from A that is of his individual decision color, he is paid an amount x, and zero otherwise.

In the experiment subjects face a choice list with 21 binary choices between the ambiguous prospect A and different risky lotteries, respectively. The risky lotteries are again represented by urns, each filled with a *known* distribution of 100 chips which are red or blue. In decision item i ($i = 1, \ldots, 21$) the respective risky urn, R_i, contains exactly $q_i \cdot 100$ red chips and $(1 - q_i) \cdot 100$ blue chips. Choosing R_i is equivalent to choosing a prospect that pays an amount x if a red chip is drawn from R_i, and zero otherwise. Hence, the color "red" takes on the same role for any R_i as the decision color for the ambiguous prospect A. Within the choice list A and x remain constant, while q_i increases monotonically.

If a subject switches from A to R_j (or vice versa), i.e., in decision item j, his probability equivalent, denoted by q, is defined by the midpoint between q_{j-1} and q_j. Thus, q approximates the probability which makes him indifferent between risk and ambiguity.[12] Comparing q and p then provides a measure for a subject's ambiguity attitude, taking into account the domain of outcome x (as explained in the following section). Using a choice list instead of eliciting direct choices between a risky (with ambiguity neutral distribution) and ambiguous urn has the particular advantage that it allows to gain a measure for the intensity of ambiguity aversion and ambiguity seeking, and to identify ambiguity neutrality. If subjects would only face a direct choice between a risky and an ambiguous prospect, then ambiguity neutral subjects might make opposite choices in Part 1 and Part 2 simply because they exhibit indifferent preferences.[13]

4.2.2 Treatment variation

Outcome domain. Expected Utility Theory still serves as the standard normative approach to model decision-making under uncertainty, as formalized by von Neumann and Morgenstern (1947). Yet, in order to explain choices under uncertainty and incorporate behavioral and cognitive biases, Prospect Theory was proposed as a descriptive approach by Kahneman and Tversky (1979) (and revised in Tversky and Kahneman, 1992). One key feature of Prospect Theory is that it assumes changes of wealth relative to a reference point, instead of total wealth, to be carriers of utility, and, moreover, that losses loom

[12]More specifically, if a subject switches in decision item $j \in \{1, \ldots, 21, \emptyset\}$, where $j = \emptyset$ denotes that he never switches at all, then his probability equivalent is defined as follows: if $j \neq \emptyset, 1$, q is the midpoint between q_{j-1} and q_j; if $j = 1$, q is extrapolated to $q_1 - \frac{1}{2}(q_2 - q_1)$; if $j = \emptyset$, q is extrapolated to $q_{21} + \frac{1}{2}(q_{21} - q_{20})$. q_i ranges from 0.26 to 0.66, increasing in steps of 0.02; one item also captures the ambiguity neutral probability $p = 0.5$ which would make an ambiguity neutral subject indifferent between choosing the ambiguous and the risky prospect.

[13]I do not explicitly consider ambiguity neutrality. The choice lists are constructed such that probability equivalents are midpoints of the intervals $[0.26, 0.28], [0.28, 0.30], \ldots, [0.64, 0.66]$, and hence should never take on value 0.5. Since I am interested in changes in ambiguity attitudes between Part 1 and Part 2, this does not constrain the analysis.

larger than gains. Such a framing effect is also commonly observed with respect to risk attitudes and the shape of the utility function which is modeled as concave in the gain domain, reflecting risk aversion, and as concave in the loss domain, reflecting risk loving preferences (Tversky and Kahneman, 1991). As previously noted, similar preference reversals have also been documented in choices under ambiguity (see, e.g., Hogarth and Kunreuther, 1985; Vieider et al., 2012).
To account for differing ambiguity attitudes in the gain and loss domain experimental treatments either involve lotteries defined on the domain of gains or losses, with $x =$ €10 in the gain, and $x = -$€10 in the loss treatments.
In the gain domain, the chance of winning x by choosing R_i increases with each decision item i. In this case, subjects might choose the ambiguous urn A for low likelihoods p_i, but switch to a risky prospect at some j. Hence, if a subject's probability equivalent q is smaller than the ambiguity neutral probability p, i.e., $q < p$, he is classified as *ambiguity averse*, and as *ambiguity seeking* if $q > p$. In the loss domain, in contrast, the chance of loosing x from choosing R_i increases with each decision item i. Hence, subjects might choose R_i for small probabilities p_i, but switch to the ambiguous urn A at some j. In this case, an individual is classified as *ambiguity seeking* if q is smaller than p, $q < p$, and as *ambiguity averse* if $q > p$.

Social anchor. In Part 2, subjects are given the opportunity to reconsider their choices from Part 1. Therefore, they are shown their own choice profile completed in Part 1. In the individual treatments any information about others' choices is excluded. In the social anchor treatments, in contrast, subjects are randomly assigned to *groups* of two. That is, each subject is randomly matched to another participant in the beginning of Part 2, whom I might refer to as a *peers* in what follows. Additionally to viewing their own choice profiles, they learn the choice profile of their peer. Screenshots of the complete choice lists of the individual as well as the social anchor treatment in Part 2 are provided in Figures D.2 and D.3, in appendix D.1. Table 4.1 summarizes the between-subjects design.

Predictions. Motivated by empirical regularities with respect to imitative behavior commonly observed in the lab (such as in the seminal study of Asch, 1956) and in the field (as previously discussed, among others, in financial decisions; e.g., Bursztyn et al., 2014), one may easily come up with the following straightforward predictions. First, if ambiguity attitudes are a stable part of individual preferences, one should expect that choices in Part 1 and Part 2 reflect the same attitudes.

$P1$: In Part 2 subjects exhibit the same ambiguity attitude as in Part 1.

Second, if peer effects are still present in decision-making under ambiguity, as identified in risky choices in the lab and in financial choices in the field, then one should expect that imitative behavior lets peers converge to each others' attitudes.

	Information (about choice profiles in Part 1 provided in Part 2)	
Outcome domain	Individual	Social anchor
Gain	$x = €10$ Own choices *GAIN-IND*	$x = €10$ Own choices + peer's choices *GAIN-PEER*
Loss	$x = -€10$ Own choices *LOSS-IND*	$x = -€10$ Own choices + peer's choices *LOSS-PEER*
	Two experimental sessions à 20 subjects per treatment	

Notes: x denotes the outcome of the binary prospect, which is different to zero; treatment labels printed in italics.

Table 4.1: Between-subjects design

*P*2: Being provided with a social anchor makes subjects shift in their ambiguity attitudes towards the peers' attitudes.

Third, if shifts occur towards a peer's attitude, then individuals should change their probability equivalents more frequently in the presence than in the absence of a social anchor.

*P*3: Being provided with a social anchor increases the frequency with which subjects change the intensity of their attitudes.

However, as becomes clear in section 4.3, the dynamics behind changes in ambiguity attitudes seem to be slightly more manifold and hard to capture by plain conjectures.

4.2.3 Experimental procedures

In the beginning of the experiment, when subjects have not seen any instructions about Part 1, participants receive an initial endowment of €10 and select their individual decision color. Participants also learn that choices from some parts might be shown to other participants in succeeding parts of the experiment. If individuals anticipate negative evaluation by others, as suggested by the "Other-evaluation hypothesis", choices might be biased towards ambiguity aversion in Part 1 and 2, in *all* treatments (as mentioned in the introduction, see Curley et al., 1986). However, subjects are also told that they remain completely anonymous throughout the entire experiment, that outcomes are private information, and that payments are made in private. Hence, I do not expect that this information significantly affects individual ambiguity attitudes.

Risky and ambiguous lotteries are explained by using opaque bags which are filled with plastic chips of different colors. To improve participants' understanding of risky distributions, the composition of colored chips of each R_i ($i = 1, \ldots, 21$) is placed on a table in the middle of the lab room, visible to participants during the experiment (see Figure D.1 in appendix D.1 for a picture). For the ambiguous prospect one opaque bag has already been filled with 100 chips at the time when subjects enter the laboratory, and is also placed on that table. I emphasize in the instructions that the distribution of chips is unknown also to me as the experimenter, and I allow participants to inspect the bag when the experiment is finished.[14]

Only Part 1 or Part 2 is randomly chosen, and within each part, only one decision item is determined to be payoff relevant. In the individual treatments, the payoff relevant part and decision item are randomly selected on the subject level. In the social anchor treatments, however, within each group, one group member is paid for Part 1 while the other is paid for Part 2, which is randomly assigned (and common knowledge from the instructions). Then, the payoff relevant decision item is randomly selected on the group level. In this way, each Part is still equally likely for each participant. Further, an individual who exhibits relative payoff concerns with respect to the peer might have an additional incentive to imitate the peer's choices – since his choices in Part 2 can perfectly fit those of the peer from Part 1. This argument holds as long as the individual dislikes social losses more than they suffer from social gains, as already assumed in the classic inequity aversion model by Fehr and Schmidt (1999).[15]

To implement the design in the most transparent way two participants are randomly selected as assistants at the end of the experiment. One assistant fills all risky bags and draws one chip from each risky and from the ambiguous bag, respectively. The other assistant enters the colors on his screen, which determines final payoffs.[16] All instructions are handed out in printed version and read aloud to subjects. Instructions for any part are not handed out before the preceding part is finished. Instructions for the gain domain, for individual and social anchor treatments, are provided in appendix D.1.

Sessions were run at MELESSA, the Munich Experimental Laboratory for Economic and Social Sciences at the University of Munich, in February 2014. The experiment was computerized using zTree (Fischbacher, 2007). In total 160 subjects participated in 8 experimental sessions, with two sessions and 40 participants per treatment. 55% were female, the average age was 24 years, and 26% were students with an economics or business background.[17] Participants earned on average €15.60 (appr. $21.30 at the time of the

[14]The composition was truly unknown to the experimenter: a student assistant blindly drew 100 chips out of an opaque bag filled with far more than 100 chips of both colors.

[15]The experimental design can be slightly extended to compare effects from learning the peer's choice profile and from exhibiting distributional preferences. For example, in the social anchor treatments payoff-relevant decision items and parts could be randomly selected on an individual level.

[16]In the interest of time, bags for R_i were only filled for decision problems 9 to 17 (symmetric around $p = 0.5$). For the remaining risky bags of decision problems 1 to 8 and 18 to 21 the computer randomly selected one color, according to the respective distribution of colored chips.

[17]Individual characteristics were balanced across treatments. Tow-sided Fisher exact tests yield p-values above conventional levels (> 0.1) with respect to compositions in gender or economic/non-economic

experiment) and the experiment lasted roughly one hour.

4.3 Results

In section 4.3.1 I first briefly comment on the dataset which is used for the empirical analysis. Results on individual ambiguity attitudes are reported in section 4.3.2. Changes in ambiguity attitudes are analyzed in section 4.3.3.

4.3.1 Data

The dataset consists of observations from 160 subjects. A subject's probability equivalent is derived from his choices in Part 1 and Part 2, denoted by q_{P1} and q_{P2}, respectively.
As common in experiments on individual decision-making that use choice lists, some subjects indicate a conflicting pattern of choices.[18] In particular, a subject might switch from prospect A to R_i (or vice versa) multiple times, since I do not enforce a single switching point in the computer program. As long as his choice in the very first and very last decision item indicates different lotteries (ambiguous or risky), q can be approximated by the midpoint of probability equivalents defined by the first and last switch.[19] However, if a subject switches an even number of times, in which case his choice in the very first and very last decision item would indicate the same type of prospect, defining a reliable approximation of q is not possible. Also, as described earlier, a subject should switch from ambiguity to risk in the gain domain, and from risk to ambiguity in the loss domain. A reversed choice pattern does not seem rationalizable and might rather suggest that the participant did not follow the task carefully enough. For the analysis presented below I drop observations of those who exhibit an even number of switching points and who show a reversed pattern of choices in either Part 1 or Part 2 (or both). This leaves us with 35 subjects in GAIN-IND and LOSS-IND, 36 in GAIN-PEER, and 38 in LOSS-PEER.[20]
In the social anchor treatments subjects can be assigned to three categories: those who are more ambiguity averse compared to their peer, those who are less ambiguity averse compared to their peer, and those who exhibit exactly the same attitude as their peer. Technically, distinguishing between these categories is only necessary in the PEER-treatments:

students. Only age differed slightly between GAIN-IND and GAIN-PEER, with average age of 25.1 compared to 22.5 years, respectively; Wilcoxon rank-sum test yields p-value 0.001.

[18]Violations of monotonicity are a common phenomenon in experiments, see, e.g., Birnbaum (1992); Birnbaum et al. (1992); Birnbaum and Sutton (1992); Charness et al. (2007); Keck et al. (2011).

[19]Although, given the construction of the choice lists, q should never take on value 0.5, one subject in Part 1 of LOSS-PEER switches multiple times, first between 0.38 and 0.40, and second between 0.60 and 0.62; taking the midpoint of both respective probability equivalents yields 0.5. Another subject in Part 2 of LOSS-IND also switches multiple times such that q is estimated as 0.5. Both subjects are classified as ambiguity seeking/neutral.

[20]Results do not change if I generally drop those who switch multiple times. Details can be received from the author.

while all choices made in Part 1 are independent observations, choices in Part 2 are not statistically independent between group members since peers learn each others' Part 1 choices. Statistical tests, however, mostly require observations to be independent. I therefore assign observations to two groups, with statistically independent observations, each. Comparing probability equivalents in Part 1, I group those subjects who are more ambiguity averse than their peer (Group 1), and those who are less ambiguity averse than their peer (Group 2); if group members exhibit the same attitude one subject is randomly assigned to one group and his peer to the other. Thus, these datasets of independent observations also differ with respect to subjects' *relative* ambiguity attitude. Where applicable I only report test results from two-sided tests, such as for t-tests or Fisher exact tests.

Lastly, a measure for cognitive ability is given by the the number of correct answers in the intelligence test (Part 3), ranging from zero to 10, with an average of 6.4 scores, and 1.4 standard deviation. Besides age, gender and field of study, I also control for cognitive ability in the regression analyses. Across treatments, cognitive ability does not differ significantly (p-values of Kruskal Wallis tests >0.2 for each treatment comparison). Neither do probability equivalents differ significantly with respect to cognitive ability (rank-sum test; p-values 0.562, 0.681, for Part 1 and 2, respectively).[21]

4.3.2 Individual ambiguity attitudes

Table 4.2 summarizes the fraction of ambiguity averse subjects and the median probability equivalents in Part 1 and Part 2, by treatment and subgroup. (Mean values of q_{P1} and q_{P2} are discussed later in section 4.3.3, but can be found in Table D.2 in appendix D.2, by treatment and subgroup.)

Within treatments, subjects predominantly exhibit ambiguity aversion in the gain domain, while the majority of subjects are classified as ambiguity seeking in the loss domain, in Part 1 and Part 2. Using t-tests and Wilcoxon sign-rank tests median probability equivalents indicate significant ambiguity aversion in the gain domain and significant ambiguity seeking in the loss domain in Part 1; p-values for all samples are provided in Table D.1 in appendix D.2. The cumulative distribution functions of probability equivalents, presented in Figure 4.1, are in turn left skewed, with means and medians below $p = 0.5$. Comparing Figure 4.1(a) for Part 1 and Figure 4.1(b) for Part 2, does not reflect considerable changes, suggesting that the distribution of ambiguity attitudes remains largely unaffected within each treatment. In fact, as given in Table 4.2, patterns in ambiguity attitudes persists to be significant in Part 2, and on the aggregate level, i.e., pooling data from Group 1 and

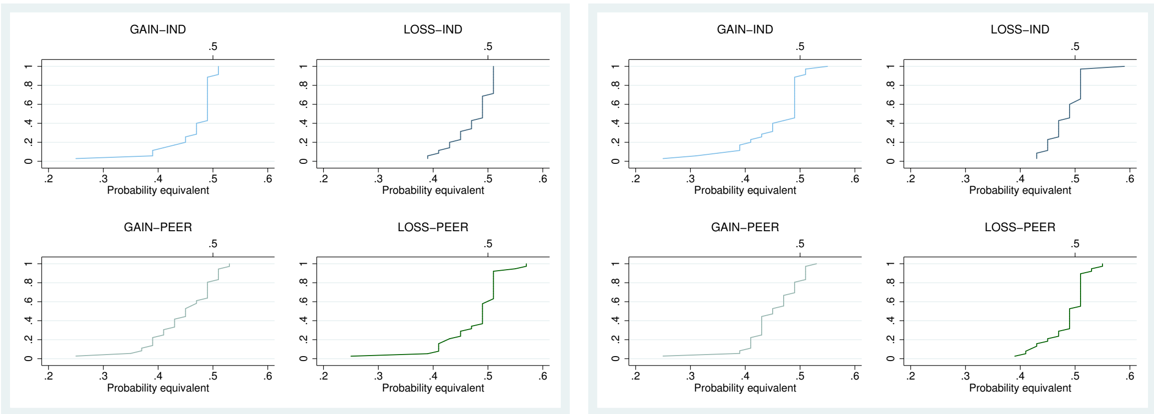
[21] Correlation coefficients between probability equivalents and the numbers of correct answers are -0.16 and 0.16 in the gain and loss domain in Part 1, respectively, and 0.07 and 0.01 in the gain and loss domain in Part 2, respectively.

Group 2 in the social anchor treatments, median values of q_{P1} and q_{P2} even coincide.

		Part 1		Part 2		% consistent between
Treatment	N	% AA	q_{P1} ^{a}	% AA	q_{P2} ^{a}	Part 1 and 2^{b}
GAIN-IND	35	88.6%	0.49 AA***	88.6%	0.49 AA***	82.9
LOSS-IND	35	31.4%	0.49 AS***	37.1%	0.49 AS***	82.9
GAIN-PEER	36	80.6%	0.45 AA***	80.6%	0.45 AA***	94.4
Group 1	19	94.7%	0.41 AA***	94.7%	0.43 AA***	100.0
Group 2	17	64.7%	0.49 AA**	64.7%	0.49 AA**	88.2
LOSS-PEER	38	39.5%	0.49 AS**	47.4%	0.49 AS**	81.6
Group 1	19	52.6%	0.51 (AA)	57.9%	0.51 (AA)	73.7
Group 2	19	26.3%	0.47 AS***	36.8%	0.49 AS**	89.5

Notes: N denotes number of observations; AA=ambiguity averse; AS=ambiguity seeking; a: median; b: classified as AA (AS) in Part 1 and in Part 2; two-sided t-test against p=0.5; *** (**, *) denotes significance on level $p < 0.01$ ($p < 0.05$, $p < 0.1$).

Table 4.2: Distribution of ambiguity attitudes



(a) Part 1 (b) Part 2

Figure 4.1: Cumulative distribution functions

However, distinguishing between those subjects who happened to be more ambiguity averse than their peer (Group 1) and those who were less ambiguity averse than their peer (Group 2), I find a slight shift towards ambiguity neutrality.[22]

Across treatments, ambiguity attitudes support the typical two-fold pattern with respect to gains and losses. On the aggregate level, the distributions of ambiguity aversion in Part 1 and Part 2 are significantly different between GAIN and LOSS treatments (χ^2-test and Fisher exact test; p-values for all subsamples are provided in Table D.3 in appendix D.2).[23] Moreover, results are more pronounced in Part 1, where differences are significant

[22]In GAIN-PEER the median probability equivalent increases by two percentage points for the relatively more ambiguity averse subjects, while it remains unchanged for the relatively less ambiguity averse. In contrast, in LOSS-PEER the median probability equivalent does not change for the more ambiguity averse subjects, who are actually not statistically distinguishable from ambiguity neutrality.

[23]It is not possible to test for differences in ambiguity attitudes by comparing probability equivalents

on the aggregate as well as for each subgroup, compared to Part 2. This variation allows to examine changes in ambiguity attitudes anchored in an aversion against, or a preference for ambiguity.

Across treatments, with respect to the feedback dimension, the distribution of probability equivalents between IND and PEER treatments does not significantly differ in Part 1, as expected (based on rank-sum tests for the distributions of probability equivalents, and χ^2-test and Fisher exact tests for the distributions of ambiguity attitudes; p-values of all tests provided in Table D.4, appendix D.2).[24]

Lastly, Table 4.2 also reports on consistency: a subject is said to be consistent if he is classified as ambiguity averse (seeking) in Part 1 and in Part 2. On the aggregate level, consistency is high and lies between 81.6% in LOSS-PEER and 94.4% in GAIN-PEER. Moreover, I do not find any systematic relationship between treatment and consistency rates. In support of prediction $P1$, the distribution of attitudes does not differ between Part 1 and 2 for any subgroup (McNemar change test; all p-values>0.1).

4.3.3 Changes in ambiguity attitudes

So far the data does not suggest substantial changes in ambiguity attitudes itself. However, changes in the intensity of ambiguity attitudes seem to be more frequent. In the following, I say that a *change* in a subject's ambiguity attitude occurs if q_{P1} differs from q_{P2}. I define a *shift* in probability equivalents as the difference $q_{P2} - q_{P1}$. This variable embodies the direction and extent of changes in attitudes.

There might be heterogeneity in the likelihood to change across treatments, which in turn might bias location parameters of shifts in ambiguity attitudes. For example, providing a social anchor might per se influence frequencies of change. I therefore examine shifts in ambiguity attitudes conditional on a change in a first step. In a second step, I examine the likelihood that subjects actually change their probability equivalent between Part 1 and Part 2.

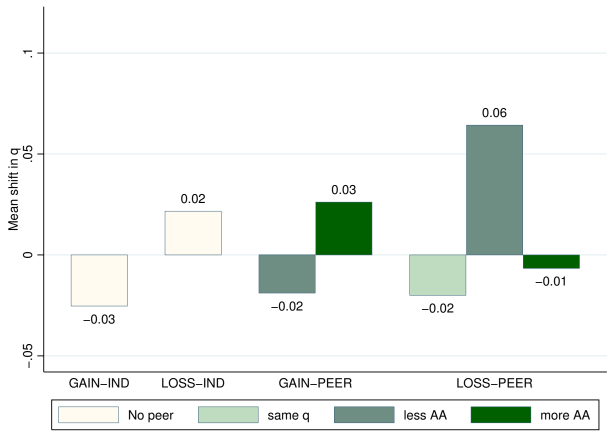
---

between GAIN and LOSS treatments. While $q < 0.5$ implies ambiguity aversion in the gain domain, it corresponds to ambiguity seeking in the loss domain. Also, since choice lists are not symmetric around 0.5, recoding q does not offer a clean approach to test for differences between outcome domains.

[24] Only in Group 1 the relatively more ambiguity averse subjects appear significantly more ambiguity averse in GAIN-PEER compared to GAIN-IND; but this holds for Part 1 as well as for Part 2 (rank-sum test, p-values 0.037 and 0.056 for Part 1 and Part 2, respectively). In order to test Group 1 and Group 2 samples of PEER treatments against comparable subsamples in the IND treatment, subjects are randomly assigned to another participant and classified accordingly. In Part 2, the distribution of ambiguity attitudes still does not differ significantly on the aggregate level.

Shifts in ambiguity attitudes

Consider only those who actually change, i.e., $q_{P1} \neq q_{P2}$, who represent 50% of the whole sample (as explicitly discussed later in this section).[25] Then, Figure 4.2 pictures the average shift in probability equivalents, distinguishing by treatment, and by relative ambiguity attitudes in the PEER treatments. In the individual treatments probability equivalents decrease on average by three percentage points in the gain domain – corresponding to a shift in 1.5 choice items in the choice list – and increase by two percentage points in the loss domain. This shift towards ambiguity neutrality is only significant in the loss domain (Wilcoxon sign-rank test; p-values 0.047).[26]



Notes: Bars are based on the following total numbers of observations. IND: GAIN-IND: 15; LOSS-IND: 18.
PEER: N=(Same q_{P1}, less AA, more AA). GAIN-PEER: (-, 9, 13); LOSS-PEER: (1, 7, 9).

Figure 4.2: Average shift between q_{P1} and q_{P2} (given change)

If a social anchor is available, average probability equivalents do not change on the aggregate in the gain domain (see Table D.2, appendix D.2). Essentially, subjects move towards their peer's attitude: those who are less ambiguity averse than their peer become more ambiguity averse, and vice versa. I observe a similar movement in the loss domain: those who were more ambiguity seeking become more ambiguity neutral. The convergence to peers' attitudes is significant when comparing the absolute difference between probability equivalents of the decision maker and his peer, between Part 1 and Part 2 (Wilcoxon sign-rank test; p-values 0.015, 0.036 for the more and less ambiguity averse in GAIN-PEER, and 0.017 for the less ambiguity averse in LOSS-PEER). Those who were more ambiguity averse (or closer to ambiguity neutrality) in LOSS-PEER move only

[25] In total, $q_{P1} \neq q_{P2}$ holds for 15 out of 35 subjects in GAIN-IND, for 18 out of 35 subjects in LOSS-IND, for 22 out of 36 subjects in GAIN-PEER, and for 17 out of 38 subjects in LOSS-PEER.

[26] I provide average values of q_{P1} and q_{P2}, by treatment and subgroup, for those who change and those who do not change in Table D.2 in appendix D.2, where I also report the number of observations in each cluster.

slightly towards ambiguity seeking, which is not significant (p-value 0.629).[27] Still, the results strongly support prediction $P2$ in the gain domain, namely that subjects tend to shift towards their peers' attitudes.

Overall, if attitudes are volatile and therefore change, then attitudes seem to move towards neutrality in individual conditions. Especially ambiguity seeking in the loss domain appears to be less robust than ambiguity aversion or neutrality, since it significantly attenuates both in the individual and social anchor condition. Moreover, imitative shifts in the gain domain suggest that a social anchor affects individual choices differently in the domain of gains compared to losses. This can be summarized as follows.

Result 1 (Shifts in attitudes conditional on changes in q)**.**

1. *Without social anchor, subjects move towards ambiguity neutrality in the loss domain. A shift towards ambiguity aversion in the gain domain is not significant.*
2. *With social anchor, subjects move towards the peer's ambiguity attitude ($P2$)*
   - *(i) towards both directions in the gain domain, but*
   - *(ii) only towards ambiguity neutrality in the loss domain.*

In a next step, I test whether these dynamics might suppress (or enforce) prevailing attitudes. Therefore, I estimate probit models which regress the likelihood that subjects become more ambiguity neutral. Interdependencies between peers are controlled for by clustering robust standard errors on the group level. Results are reported in Table 4.3.

In the gain domain, being provided with a social anchor significantly increases the likelihood for a shift towards ambiguity neutrality, relative to the individual treatment as a baseline. This suggests that the slight shift towards aversion in GAIN-IND is counterbalanced in GAIN-PEER, where subjects tend to move towards the peer's attitude, in both directions. It also appears that the ambiguity averse are significantly more likely to move towards neutrality, which is likely due to the scope for becoming even more ambiguity averse being limited. However, this effect is significantly stronger in the IND compared to the PEER condition suggesting that individual attitudes play a more prominent role if a social anchor is not available (model (3); Wald test between coefficients of AA-IND and AA-PEER, p-value < 0.01).

In the loss domain, in contrast, there is no significant difference in the likelihood to move towards ambiguity neutrality between PEER and IND treatments. This is consistent with the significant shifts towards neutrality in both treatments. Mirroring the previous finding from the gain domain, ambiguity aversion makes a shift towards ambiguity neutrality

[27] Similarly, shifts are significantly different from zero for the relatively more ambiguity averse in GAIN-PEER, Wilcoxon sign-rank test, p-value 0.010; for the relatively less ambiguity averse in GAIN-PEER and LOSS-PEER, p-values 0.036 and 0.024; but not for the relatively more ambiguity averse in LOSS-PEER, p-value 0.465.

shift → AN	(1) GAIN	(2) GAIN	(3) GAIN	(4) LOSS	(5) LOSS	(6) LOSS
PEER	0.378**	0.373**	1.354***	-0.057	0.078	0.162
	[0.131]	[0.131]	[0.285]	[0.148]	[0.146]	[0.162]
AA		0.266			-0.454***	
		[0.172]			[0.110]	
AA-IND			1.225***			-0.221
			[0.244]			[0.231]
AA-PEER			0.218			-0.575***
			[0.214]			[0.137]
Cogn. ability	0.146***	0.109***	0.108**	-0.016	-0.011	-0.014
	[0.037]	[0.041]	[0.042]	[0.056]	[0.040]	[0.039]
Observations	37	37	37	35	35	35
Log.-lik.	-18.38	-17.62	-17.50	-20.79	-16.27	-15.65

Notes: Marginal effects of a probit regression on the likelihood to become more ambiguity neutral; independent variables include a dummy variable for PEER treatments, where IND treatments are the baseline category, a dummy variable for AA(-IND, -PEER)=ambiguity aversion (in IND, PEER treatment), and the measure for cognitive ability. I control for gender, age and economic/business studies. Robust standard errors, clustered on group level; *** (**, *) denotes significance on level $p < 0.01$ ($p < 0.05$, $p < 0.1$).

Table 4.3: Probit model for shifts towards ambiguity neutrality

significantly less likely compared to ambiguity seeking. In this case, being classified as ambiguity averse corresponds an average probability equivalent of 0.53, with median 0.51, i.e., these subjects are relatively close to neutrality already. Moreover, the effect appears to be driven by the PEER treatment (model (6)). Yet, this might be confounded by a paradigm that is shown in the next section: the majority of subjects who actually change in LOSS-IND is ambiguity seeking already (in contrast to LOSS-PEER), hence, providing more scope for shifts towards neutrality.

Apparently, being of high cognitive ability significantly increases the likelihood for a shift towards neutrality in the gain domain suggesting a cognitive component within ambiguity attitudes, whereas marginal effects are of negligible and insignificant size in the loss domain. Hence, while ambiguity seeking migh instinctively decline, biases towards ambiguity aversion might be overcome by rational reasoning.

Overall, complementing the previous findings, the provision of a social anchor results in different dynamics in the domain of gains compared to losses.

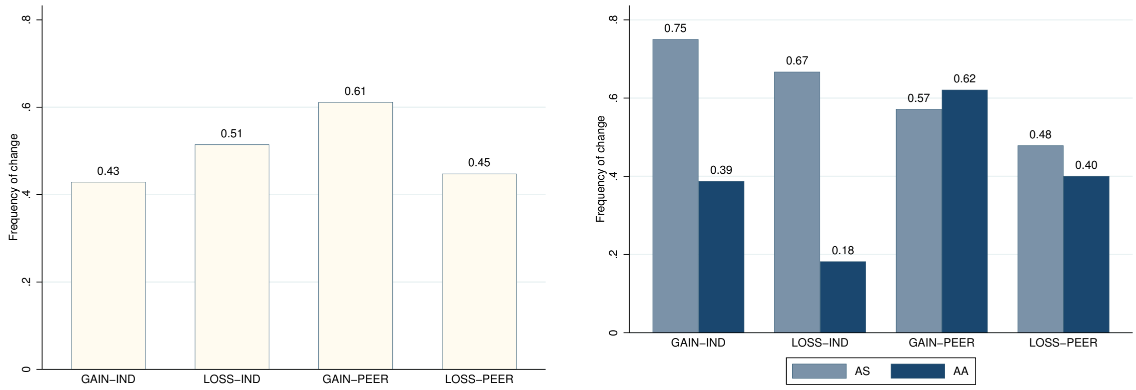
Result 1 (Shifts in attitudes conditional on changes in q (cont'd.))**.**

3. *In combination, a social anchor significantly increases the likelihood for a shift towards ambiguity neutrality in the gain, but not in the loss domain.*
4. *Cognitive ability significantly and positively correlates to shifts from ambiguity aversion towards neutrality in the gain domain, while it is not of relevance in the loss domain.*

Frequencies of change

So far, I examined effects conditional on changes in probability equivalents. But how frequent are changes actually? And how do frequencies of change relate to treatment variations? At a first glance, I observe an average frequency of change of 50% over the whole sample. Thus, the intensity of ambiguity attitudes is likely to fluctuate, also if no social anchor is available.

Figure 4.3 reports the average frequency of change, distinguishing by treatment in subfigure (a), and by treatment and ambiguity aversion in subfigure (b). Figure 4.3(a) again



(a) By treatment (b) By treatment and ambiguity attitude

Notes: Bars are based on the following total numbers of observations. (a) GAIN-PEER: 35; LOSS-IND: 35; GAIN-PEER: 36; LOSS-PEER: 38. (b) N=(AS, AA). GAIN-PEER: (4, 31); LOSS-IND: (24, 11); GAIN-PEER: (7, 29); LOSS-PEER: (23, 15).

Figure 4.3: Average frequency of change

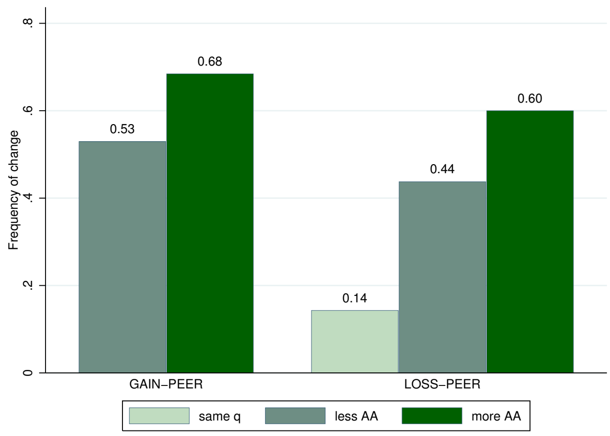
suggests that social anchor effects differ in their nature between gain and loss domains. Changes are more frequent in the gain domain if a social anchor is available compared to the individual condition. This finding exactly reverses in the loss domain, where changes are in fact less frequent if a social anchor is available compared to the individual condition. Hence, there is no general support for prediction $P3$, namely that the presence of a social anchor makes changes more likely.

However, turning to Figure 4.3(b), frequencies of change seem to be systematically correlated with individual ambiguity aversion in the individual treatments, independently of the outcome domain. While 75% and 67% of the ambiguity seeking subjects change in the gain and loss domain, respectively, this frequency drops to 39% for the ambiguity averse subjects in the gain domain, and to only 18% for the ambiguity averse subjects in the loss domain. The difference is significant for LOSS-IND (χ^2-test and Fisher exact test; p-values 0.008 and 0.012; p-values for all subsamples considered in Figure 4.3 provided in Table D.5, appendix D.2), and remains significant on the aggregate level, pooling GAIN-

IND and LOSS-IND (p-values 0.005, 0.007).[28] In contrast, Figure 4.3(b) also shows that frequencies of change are not systematical and less affected by ambiguity aversion if a social anchor is available.

Why does the relationship between an individual's ambiguity aversion and the likelihood to change vanish in the presence of a social anchor? Figure 4.4 pictures the frequencies of change in both PEER treatments, distinguishing by *relative* ambiguity aversion, i.e., between those who are more ambiguity averse, and those who are less ambiguity averse than their peer (who might also be framed as more ambiguity seeking in the loss domain). In LOSS-PEER, there are also four pairs of peers who indicated the same probability equivalents, which does not occur in GAIN-PEER.[29]

I observe a monotonous relationship between relative ambiguity aversion and the likelihood to change. The finding that subjects who are more ambiguity averse than their peer change more frequently than subjects who are less ambiguity averse, parallels the previous result that ambiguity aversion generally tends to decrease frequencies of change in the individual treatments.



Notes: Bars are based on the following total numbers of observations, with N=(same a. attitude, less a. averse, more a. averse). GAIN-PEER: (0, 17, 19); LOSS-PEER: (7, 16, 15).

Figure 4.4: Average frequency of change by relative ambiguity attitude

Do these patterns with respect to individual and relative ambiguity aversion possibly interact? To answer this question I estimate probit models which regress the likelihood to change on the presence of a social anchor using the individual condition as the baseline

[28] The difference is not significant in GAIN-IND (p-values 0.167, 0.292). This might be partially due to the fact that the distribution of ambiguity averse and ambiguity seeking subjects is skewed: in GAIN-IND only 11.4% are ambiguity seeking, while the vast majority of 88.6% are ambiguity averse (see Table 4.2). In contrast, in the distribution in LOSS-IND is more symmetric, with 68.6% being ambiguity seeking and 31.4% being ambiguity averse. If there is a generally negative relationship between ambiguity aversion and the variability of ambiguity attitudes (in the absence of a social anchor), this might (partly) explain why changes are on average less frequent in the gain compared to the loss domain.

[29] In one of these groups, one subject was dropped due to violation of a consistency criterium; thus Figure 4.4 only covers seven subjects in this group.

category (model (1)); controlling for ambiguity aversion of the decision maker (2); and allowing for the possibility that ambiguity aversion has a different impact in the IND compared to the PEER condition (3). Then, I estimate the impact of learning to be less or more ambiguity averse compared to the peer, relative to the baseline of having no social anchor at all (models (4) - (6)).[30]

There are sizable effects in the gain domain; results are reported in Table 4.4. The provision of a social anchor increases (though only close to marginal significance) the likelihood to change, consistent with Figure 4.3(a). Further, being more ambiguity averse than the peer has a positive marginal effect on the likelihood to change relative to the individual condition, significant at the 10% level. Consistent with Figure 4.4, this does not apply for learning to be less ambiguity averse.[31] This is also consistent with Result 1 *3.*, namely that a social anchor increases shifts towards ambiguity neutrality. While individual ambiguity aversion does not significantly matter, high cognitive ability again has a significant and negative marginal effect on the likelihood to change. This is particularly striking given that initial probability equivalents and cognitive ability are not significantly correlated. Hence, it rather suggests that the confidence in the "correctness" of one own's choices in the standard Ellsberg (gain) setting might correlate with ability in other cognitive tasks.

Turning to the loss domain, results are presented in Table 4.5. In line with Figure 4.3(b), ambiguity aversion decreases the likelihood to change, but significantly so only in the individual condition. Again, being provided with a social anchor generally has no significant impact (models (1)-(3)). This suggests that ambiguity seeking goes along with a higher variability of attitudes; but this relation fades out as soon as a social anchor becomes available.[32] Further, I observe a *reassurance effect*: compared to the baseline of having no social anchor available, those peers who exhibit exactly the same ambiguity attitude are less likely to change. Instead, those who are less or more ambiguity averse than their peer are not significantly different in terms of their likelihood to change, compared to IND. Hence, realizing that attitudes towards ambiguity are similar might serve as a confirmation device. Reflecting Figure 4.4, marginal effects are significantly different comparing those who are more ambiguity averse to those who have the same attitude as their peer.[33] Since the impact of individual ambiguity aversion only significantly matters in IND, the reassurance effect vanishes if I only control for a subject's ambiguity aversion (model (5)) but not allow for differences between a subject's ambiguity aversion in IND and PEER

[30]Coefficients of interaction terms in non-linear models might be biased in sign (see Ai and Norton, 2003). Thus, I only compare ambiguity aversion in the IND and PEER treatments to ambiguity seeking in all treatments. Additionally, I estimated OLS regressions which yield qualitatively similar results.

[31]Coefficients are, however, not significantly different between PEER×less AA and PEER×more AA; linear hypotheses tests, p-values>0.2.

[32]Coefficients of AA-IND and AA-PEER are significantly different based on linear hypothesis test; p-values 0.094, 0.041, 0.063 for models (3), (6), and (8), respectively. I observe the same relationship in the gain domain (Table 4.4), although effects are not significant in that case.

[33]Linear hypothesis test; p-values=0.056, 0.035 in models (4) and (6).

Likelihood to change	GAIN domain (1)	(2)	(3)	(4)	(5)	(6)
PEER	0.152	0.153	-0.184			
	[0.106]	[0.105]	[0.267]			
PEER				0.051	0.044	-0.204
× less AA				[0.137]	[0.142]	[0.220]
PEER				0.242*	0.244*	-0.048
× more AA				[0.132]	[0.134]	[0.307]
AA		0.017			-0.036	
		[0.142]			[0.161]	
AA-IND			-0.217			-0.221
			[0.254]			[0.252]
AA-PEER			0.173			0.099
			[0.139]			[0.189]
Cogn. ability	-0.110**	-0.111**	-0.111**	-0.118**	-0.115**	-0.114**
	[0.039]	[0.042]	[0.041]	[0.038]	[0.041]	[0.040]
Observations	71	71	71	71	71	71
Log.-lik.	-44.15	-44.15	-43.41	-43.44	-43.42	-42.94

Notes: Marginal effects of a probit regression on the likelihood to change; independent variables are given as in Table 4.3; dummies PEER×less (more) AA indicate whether the individual is less (more) ambiguity averse than his peer; controls for gender, age and economic/business studies are included. Robust standard errors, clustered on group level; *** (**, *) denote significance on levels $p < 0.01$ ($p < 0.05$, $p < 0.1$).

Table 4.4: Probit regression for the GAIN domain

(as in model 6). However, I also need to note that those peers who have the same ambiguity attitudes were predominantly ambiguity averse (6 subjects), compared to ambiguity seeking (only 1 subject), which might also drive the overall effect of ambiguity aversion in model (5). As a robustness check, I exclude observations of peers with identical ambiguity attitudes (models (7)-(8)), and the previous result remains unchanged: ambiguity aversion significantly decreases frequencies of change in the absence, but does not play a significant role in the presence of a social anchor.

Result 2 (Frequencies of change)**.**

1. *In the absence of a social anchor individual attitudes matter for the likelihood of change: ambiguity averse subjects change less frequently compared to ambiguity seeking subjects; this is significant in the loss domain.*
2. *The presence of a social anchor affects the likelihood of change in particular settings:*
   - *(i) being more ambiguity averse than the peer increase the likelihood to change in the gain domain;*
   - *(ii) having exactly the same ambiguity attitude as the peer significantly decreases the likelihood to change in the loss domain.*

Likelihood to change	LOSS domain (1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PEER	-0.050	-0.036	-0.161					
	[0.121]	[0.117]	[0.134]					
PEER				-0.059	-0.253	-0.206	-0.114	-0.194
× less AA				[0.163]	[0.198]	[0.160]	[0.163]	[0.162]
PEER				0.075	-0.114	-0.077	0.112	-0.060
× more AA				[0.154]	[0.159]	[0.175]	[0.154]	[0.176]
PEER				-0.352*	0.107	-0.476**		
× same AA				[0.160]	[0.156]	[0.156]		
AA		-0.225**			-0.222*		-0.252*	
		[0.102]			[0.111]		[0.120]	
AA-IND			-0.445**			-0.432**		-0.442**
			[0.169]			[0.160]		[0.166]
AA-PEER			-0.066			0.024		-0.004
			[0.129]			[0.143]		[0.157]
Cogn. ability	-0.015	-0.004	0.000	-0.011	0.003	0.003	0.003	0.003
	[0.033]	[0.033]	[0.032]	[0.035]	[0.035]	[0.034]	[0.040]	[0.040]
Observations	73	73	73	73	73	73	66	66
Log.-lik.	-48.63	-46.88	-45.62	-46.73	-45.34	-43.69	-42.10	-40.76

Notes: Marginal effects of a probit regression on the likelihood to change; independent variables are given as in Table 4.4, PEER×same A-attitude is a dummy for whether the subjects has exactly the same ambiguity attitude as his peer; controls for gender, age and economic/business studies are included. Robust standard errors, clustered on group level; *** (**, *) denote significance on levels $p < 0.01$ ($p < 0.05$, $p < 0.1$).

Table 4.5: Probit regression LOSS domain

To evaluate the predictive power of the reported regressions I contrast predicted and true values for models (1)-(6) (reported in Table D.6, appendix D.2). Model (6) indeed performs best. While (4) and (5) overestimate the likelihood to change in IND for the ambiguity averse, and underestimate the likelihood to change for the ambiguity seeking subjects, the predictions for the likelihood to change conditional on the relative ambiguity aversion in the PEER treatments remain the same across models (4)-(6).

Given that ambiguity seeking subjects in the loss domain are significantly more likely to change in the individual condition, this attitude might not be particularly robust. Moreover, individual attitudes might even predict the likelihood to change, however, only if a social anchor is not available. For gains and in the presence of a social anchor, those who learn to be more ambiguity averse than their peers switch more often. Based on Result 1 which states that decision makers tend to shift towards their peers' attitudes, this is likely to induce a shift towards neutrality on the aggregate level. Nevertheless, as noted earlier in section 4.3.2, actual attitudes are indeed quite stable, especially given that the distributions of ambiguity averse and ambiguity seeking subjects do not differ considerably between Part 1 and Part 2.

4.4 Conclusion

In this paper I provide new experimental evidence on the effect of being provided with a social anchor on attitudes towards ambiguity. Hereby, I focus on the standard Ellsberg setting (Ellsberg, 1961), with an ambiguity neutral probability of 50%, for gains and losses. In the experiment probability equivalents are elicited twice, in two consecutive rounds, individually in Part 1, and again in Part 2. In the social anchor treatments, subjects are provided with the choice profile of another participant from the first part when making their choices a second time. To distinguish between the impact of a social anchor and a general anchoring effect, everyone is shown his own complete choice profile from the first part, in the individual and social anchor treatments.

My results generally support the common two-fold pattern of individual ambiguity attitudes, indicating significant ambiguity aversion in the gain domain, and significant ambiguity seeking in the loss domain. These preferences appear stable in the sense that if individuals exhibit ambiguity aversion (seeking) in the first part, roughly 90% also do so in the second part, independent of any treatment variation. In contrast, the intensity of these attitudes does not prove to be as robust; in 50% of all cases subjects' probability equivalents do not coincide between Part 1 and Part 2.

In some respects, the availability of a social anchor seems to have rather weak effects. For example, receiving a social anchor does not significantly alter the likelihood with which subjects change their probability equivalents. In other respects, peers seem to be important. For example, conditional on a change, I mostly observe shifts towards ambiguity neutrality in the individual treatments, while subjects are very likely to converge towards their peer's attitude in the social anchor treatments. However, the analysis suggests that individual dynamics as well as peer effects differ considerably between gains and losses.

In the domain of gains, the individual's ambiguity attitude does not significantly influence the likelihood to change nor the likelihood to shift towards neutrality, neither in the individual nor in the social anchor treatment. However, learning to be more ambiguity averse than a peer significantly increases the likelihood to change, relative to having no social anchor available. That is, the *relative* ambiguity attitude, i.e., the ambiguity attitude compared to the peer's, matters. Further, conditional on a change in probability equivalents, decision makers tend to follow their peer's attitude, towards ambiguity aversion, seeking, or neutrality. Ultimately, receiving a social anchor in the gain treatments predominantly induces comparably ambiguity averse subjects to shift towards ambiguity neutrality.

In the domain of losses, in contrast, individual attitudes matter in the individual treatment, such that ambiguity seeking subjects are significantly more likely to change compared to ambiguity averse subjects. But this relationship breaks down when a social anchor becomes available, where it is again the relative ambiguity attitude that matters, but in a way that learning to have the same attitude as the peer significantly reduces the likelihood to change. I label this a *reassurance effect*. Generally, again in contrast to the gain domain, conditional on a change in probability equivalents, I observe significant shifts only from ambiguity seeking towards neutrality in the loss domain, with and without social anchor, suggesting that ambiguity seeking might not be very robust over time in such settings. This is in line with mixed evidence on ambiguity attitudes in the domain of losses, where some experiments report neutrality while others report a preference for ambiguity.

The persistent finding that cognitive ability significantly and positively correlates to shifts towards neutrality in the standard Ellsberg setting over gains further suggests that the common finding of ambiguity aversion might be driven by bounded rationality of subjects. The fact that no correlation between cognitive ability and ambiguity seeking over losses is found corroborates the conjecture that this attitude might rather be of instinctive and flighty nature.

In summary, I derive three main conclusions. The intensity of ambiguity attitudes is likely to fluctuate, even if no social anchor is available; nevertheless, peers seem to be important for decision-making under ambiguity; and being provided with a social anchor seems to affect individual attitudes differently in the domain of gains compared to losses.

In the introduction I noted that this paper is closely related to the studies by Keck et al. (2011) and Charness et al. (2013), who examine changes in individual ambiguity attitudes after consultation with others. Both studies agree in their result that ambiguity attitudes shift towards neutrality as a result of social interaction. Concordantly, the authors of both studies agree in their hypothesis that ambiguity neutrality might be perceived as a persuasive argument (Burnstein and Vinokur, 1977). The present study replicates this shift towards ambiguity neutrality in the presence of others, but reports two additional findings. First, although the relative ambiguity attitude matters for the likelihood to change, ambiguity aversion over gains might also be corroborated since individuals seem to converge to the peer's attitude in either direction, conditional on a change. Second, in the domain of losses, the shift towards ambiguity neutrality might be present whether or not a social anchor is available.

A crucial difference between this and the other two studies lies in the way in which feedback about others is introduced. The present experiment only provides subjects with information about others choices, while the cited papers allow for face-to-face interaction between peers. Thus, although feedback about others' choices might already cause peer effects, establishing ambiguity neutrality as a persuasive argument in individual choices might be not as evident if the social anchor purely refers to hard-coded information instead of discussion or consultation.

The present study is restricted to ambiguity attitudes with respect to symmetric and moderate probabilities. Future research is needed to uncover dynamics behind peer effects in situations in which events occur with small likelihoods, where hopes or fears might drive individual choices (following the terminology of Viscusi and Chesson, 1999). Moreover, in this study I do not disentangle any effects which occur through distributional preferences with respect to the peer from effects that stem from learning the peer's choices. The fact that I do observe relatively weak social anchor effects might suggest that either channel does not have a substantial impact on individual choices. However, studies on peer effects in risk taking actually suggest that distributional and informational channels might be orthogonal (see, e.g., Cooper and Rege, 2011; Bursztyn et al., 2014), i.e., induce different shifts in attitudes independently of each other. Thus, disentangling distributional and informational channels in social anchor effects might be worthwhile to understand how ambiguity attitudes can be shaped by information about others.

This study has important implications with respect to economic behavior where discriminating ambiguity from risk may have detrimental effects. Ambiguity aversion, in particular, has been proposed among economic theorists to explain suboptimal choices in decisions under uncertainty, such as with respect to the stock market participation puzzle (Easley and O'Hara, 2009) or the reluctance to take up (costless) genetic tests (Hoy et al., 2014). In this respect, my study suggests that social interaction – and even individual dynamics over time – might establish neutral preferences towards ambiguity, which might ultimately inhibit adverse effects in decision-making under uncertainty. Finally, my findings suggest that individual dynamics as well as peer effects in ambiguity attitudes might work differently in different outcome domains, and, similarly, that cognitive ability is likely to affect the evaluation of ambiguous events only in some specific settings. Obviously, further research is needed to fully understand in which settings consultation with others or providing professional advice might be particularly beneficial.

Chapter 5

Team Reasoning as a Guide to Coordination*

5.1 Introduction

It has always been transparent to the attentive observer that traditional rational choice theory (RC) performs poorly as a descriptive theory of human behavior in many areas of social interaction. In recent years, experiments with simple dilemma games by behavioral economists and other behavioral scientists validated this simple truth. The theory of team reasoning (TR; Sugden, 1993, 2000, 2003; Bacharach, 1999, 2006) is one attempt to cope with this problem. A particular problem of traditional RC is that it cannot explain equilibrium selection in simple coordination games. Patterns of coordination very often seem evident from a common sense point of view, and, so, coordination is in fact rarely an actual problem for real individuals. Still, if actors were to ask RC for advice they would get utterly frustrated. RC remains silent if equilibrium selection is the issue. If individuals look at the problem from a team perspective, however, some of the most common routes to coordination become unveiled. If there is a single pattern of behavior that is best for the group (which often amounts to 'best for each individual in the group'), then it seems reasonable that each should do his[1] part in the scheme of actions so defined. But a team perspective is not comprehensible within the classical rational choice approach to social interaction.

TR is an attempt to fill this gap by implementing team agency into the theory of rational action. Teams are added to the theory as possible agents, while the fundamental assumptions of RC about rational decision-making remain unchanged in principle. In TR, these assumptions are just transferred to the realm of the new agents and, thus, apply to individuals and groups alike. TR introduces team agency in two steps. First, team

*This chapter is based on joint work with Bernd Lahno.

[1]We refer to a decision maker as "he" throughout the paper.

preferences are defined as a common scheme of evaluation. Second, a choice rule specifies how an individual is to contribute to maximizing team utility by making his (individual) choice.

Extending agency from individuals alone to individuals and groups seems in fact suitable to account for various instances of successful coordination and other cooperative regularities. Moreover, by reference to team agency, TR can explain the stability of behavioral patterns and behavioral expectations. It thus gives some account of social norms not available within RC. However, as TR sticks with the fundamental assumptions of instrumental rationality as conceptualized in RC it also inherits the idea of opportunistic choice. As individuals according to RC, teams according to TR instantly detect and take opportunities as soon as they arise. Choices of team members, therefore, are assumed to be principally unaffected by normative expectations or behavioral regularities observed in the past. They may conform with customs or norms to the extent that those actually promote the team interest. But neither customs nor norms constitute a fundamental restriction to decision-making.

An interesting consequence of opportunism as embodied in TR's choice rule is that individuals do not make use of the entire private information available to them. While the principles of individual opportunistic rationality would demand that each individual uses all the private information available to him, a team cannot make a decision dependent on information that is available to some but not all its members. As a result, the optimal scheme of action in terms of the team goal may be different depending on whether determined from an individual or from the team perspective.

In this paper we analyze and discuss the solution concept for common coordination problems as incorporated in TR. Special consideration is given to TR's concept of opportunistic choice and to the resulting restrictions in using private information. We report results from a laboratory experiment in which we analyze behavior in simple coordination dilemmas. In particular, we test whether 'teams' react opportunistically to changes in a strategic environment.[2] In the experiment teams were given a chance to coordinate on a particular pattern of behavior in a sequence of HiLo games. Then, a modification of the stage game offered opportunities to improve on the team goal by changing this accustomed pattern of behavior. We implemented three treatments which differ with respect to the particular modification and the optimal strategy induced by team reasoning.

Our observations throw considerable doubt on the idea of opportunistic team reasoning as a guide to coordination. Contrary to what TR would predict, individuals tend to stick to accustomed behavioral patterns. Moreover, we find that individual decisions are at least

[2] Experimental studies of the empirical validity of the theory of team reasoning seem to be rare. In an experimental comparison of TR and Cognitive Hierarchy, Bardsley et al. (2010) found mixed evidence for the explanative power of TR in coordination problems. Colman et al. (2008) found evidence for team reasoning in experiments with lifelike vignettes as well as in abstract games.

partly determined by private information not accessible to all members of a team, which violates TR's assumption that individual solely use information which is available to all team members. Alternative theories of choice, in particular cognitive hierarchy theory (Camerer et al., 2004), may be more suitable to explain the observed pattern of behavior. The remainder of this paper is organized as follows. First, we briefly sketch the guiding ideas and basic elements of the theory of team reasoning. We illustrate the theory by discussing TR's suggestions to solve problems of cooperation as represented by the prisoners' dilemma and general problems of coordination as exemplified in a simple HiLo game (section 5.2). After specifying the empirical claims incorporated in TR as a descriptive theory (section 5.3) we investigate the theoretical relationship between RC and TR (section 5.4). We argue that TR inherits a strict conception of instrumental rationality and opportunistic choice from RC. To extract the opportunistic nature of TR's concept of rational choice we consider variants of the HiLo game and derive behavioral predictions under different forms of uncertainty (section 5.5). We then test the empirical significance of TR by actually implementing these cooperation dilemmas in a laboratory experiment; we describe the experimental design, and present and discuss our results (section 5.6). Finally, we conclude and provide some general remarks (section 5.7).

5.2 The foundations of team reasoning

The core idea of TR is to abandon the assumption of exclusively individual agency in RC and amend traditional decision theory by an adequate account of collective agency. Consider a classic Prisoners' Dilemma (PD) as given in Figure 5.1 (payoffs normalized):

	C	D
C	a | a	0 | d
D	d | 0	1 | 1

$d > a > 1$

Figure 5.1: Prisoners' dilemma

RC implies that rational individuals will mutually defect, (D, D), although both prefer mutual cooperation, (C, C). There is ample evidence that real decision makers in fact cooperate in such situations to a considerable extent. TR offers the following explanation: individuals do not always maximize their expected individual utility given what they believe about the actions of others as RC would demand. The individual actor may perceive a situation primarily as one that poses a problem to the group (the 'team') of the two actors as a whole rather then to each of them separately and in isolation. So, instead of asking 'What should I do?' the guiding question in the decision-making process is: 'What should we do?'. In a PD as given above the natural answer seems to be: 'We should cooperate!' meaning that the collective of the two actors should collectively realize the result (C, C). So both should do their part in the collective scheme to realize the collectively preferred result, each should cooperate. This decision-making procedure presupposes that there is a common scheme of evaluating the consequences of interaction from a team perspective. Given such a scheme, then, the question 'What should we do?' can be answered in the standard instrumental way by identifying the combination of actions that yields the best result according to team evaluation. TR prescribes that each individual does his part of the optimal scheme provided that it is common knowledge among the individuals that each individual identifies with the group and endorses the common evaluation scheme. Consequently, acting as a 'teamer' involves being guided by the team objective and a distinctive way of reasoning. TR therefore comprises two discernible parts: (1) a theory of preference transformation and (2) a decision-making rule.

It is, in fact, an extremely complex and difficult task to give a general account of how to derive team preferences from individual preferences and the relevant properties of a situation of choice. Nevertheless, in many situations we have quite firm intuitions about group evaluation. In our PD a plausible transformation from individual to team payoffs is given in Figure 5.2.

$$\begin{array}{c|c|c|} & \text{C} & \text{D} \\ \hline \text{C} & a \mid a & 0 \mid d \\ \text{D} & d \mid 0 & 1 \mid 1 \\ \hline \end{array} \quad d > a > 1 \qquad \longrightarrow \qquad \begin{array}{c|c|c|} & \text{C} & \text{D} \\ \hline \text{C} & a & e \\ \text{D} & e & 1 \\ \hline \end{array} \quad a > 1 > e$$

Figure 5.2: Team payoffs in a prisoners' dilemma

From the team perspective (C, C) is preferred to (D, D) and – in contrast to individual evaluation as given in the original matrix – also preferred to the asymmetrical outcomes (C, D) and (D, C). If all individuals take on the team perspective and if this is commonly known among them, team reasoners will proceed according to the following decision-making rule:

Choose your part of the common scheme of actions that maximizes team payoff. (tr)

One might wonder whether the choice rule of team reasoning as specified in (tr) may play a relevant role beyond traditional rules of individual rationality at all.[3] Will it not suffice to assume that it is common knowledge that both players take on the team's objective? Will rational individuals in the traditional sense of RC, maximizing the so defined utility individually, not 'automatically' decide in exactly the same way as proposed by TR without the necessity to refer to a special decision rule? The answer is NO.

[3] Although (tr) will inevitably produce cooperation in a PD it does not necessarily define an action for every individual in each and every strategic situation. If no unique optimal strategy profile exists (e.g., because of indifference in terms of team evaluation), then, obviously, an additional choice rule is needed to determine the scheme of actions to be collectively chosen.

Consider again the payoffs in the matrix to the right of Figure 5.2 and assume that the payoffs represent team as well as individual utilities derived from the respective action profiles. The payoff e is assumed to be smaller than 1, which is a plausible assumption for team evaluation. According to standard RC this game has two equilibria, (C, C) and (D, D). As we shall argue in a moment, the resulting equilibrium selection problem cannot be solved on the basis of the standard assumptions of instrumental rationality alone. The choice rule (tr) solves this problem.
The problem of equilibrium selection in RC – for which TR offers a (partial) solution – is a general one. It is just as relevant as the notorious problem of cooperation in dilemma games but has not received the same sort of attention (as it deserves). Thus, taking a closer look is worthwhile.
For illustration consider the following coordination game in Figure 5.3 (payoffs again normalized), which is known as the HiLo game.

	red		*blue*		
red	a	a	0	0	$a > 1$
blue	0	0	1	1	

Figure 5.3: HiLo game

Compared to the PD HiLo has an important property relevant to someone who is interested in the empirical validity of TR, namely, the preferences of both players are perfectly congruent. It is only natural to assume that team payoffs coincide with individual payoffs (see Figure 5.4) and the problem of deriving team preferences does not even evolve.[4]

	red		*blue*		
red	a	a	0	0	$a > 1$
blue	0	0	1	1	

$\longrightarrow$

	red	*blue*	
red	a	0	$a > 1$
blue	0	1	

Figure 5.4: Team preferences in a HiLo game

Hence, HiLo is particularly suited to investigate the empirical validity of (tr), the team reasoning choice rule. We will concentrate on variants of this game in what follows. Consider, the HiLo in Figure 5.3 from a pure RC point of view. The game has two equilibria *(red, red)* and *(blue, blue)* with *(red, red)* dominating *(blue, blue)* in the Pareto sense. So *red* seems to be the unique rational choice in this situation (our experiment once more supports the conjecture that individuals consistently act in accordance with this simple insight). But notice that the Pareto-optimality of *(red, red)* is a property of a common scheme of action, which no individual can realize individually by his choice.[5]

[4]Moreover, we may abstain from analyzing the role of (outcome-based) social preferences (see, e.g., Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000). Obviously, concerns for distributions in outcomes do not influence utilities.

[5]The same is true of the property of being an equilibrium, and a similar argument applies (Gintis, 2009).

Being solely based on conditions of individual rationality RC cannot substantiate reasons to choose *red* or *blue* in the following situation.
Consider an individual A playing against B. From an RC point of view A has reason to choose *red* only if he has reason to believe that B will also choose *red* (with sufficient probability). But B is in the same position. Knowing that B is rational like herself A therefore needs a reason to believe that B has reason to believe that A will choose *red*. But again: B is in the same position... Obviously this leads into an endless regress.
To assume that individual rationality as defined in RC might provide a decisive reason to act in certain ways – and, thus, provide a solution to the choice problem – in coordination problems as HiLo is void.[6] In contrast, TR provides an outright solution to the problem that fits very nicely with our intuitions. The fact that individual goals are completely congruent gives sufficient reason to assume that A and B realize their collective (joint) dilemma. (tr) is the core of this solution: the best result from everyone's point of view is brought about by mutually choosing *red*. And this is common knowledge. So, according to TR each has reason to follow (tr) and choose his part in the preferred scheme; each chooses *red*.

5.3 Team reasoning as a descriptive theory

TR is particularly relevant for situations in which choice may be conceived as an issue of a collective rather than of individuals. But even if a situation may be characterized as such from the outside, TR does not necessarily demand that individuals actually employ team reasoning. Team reasoning is conditional on a certain perception of the situation from inside, i.e., by the individuals that are to form the collective. It presupposes that individuals conceive themselves as members of a team and perceive this team as a possible unit of (collective) agency characterized by its team goals (cf. Gold and Sugden, 2007, 125). The essential element of such group identification then is that individuals take on the team goal as their own.
Whether or not an individual identifies with a team is a matter of framing, not a matter of rationality. Both Sugden and Bacharach agree that there are empirical regularities such that group identification is promoted by certain situations but prevented by others.[7] But these issues are understood as the object of empirical research in psychology or behavioral science, rather than as being an integral part of TR. Sugden takes TR – just like RC – as a theory without empirical content (Sugden, 2000, 203). Bacharach, however, identifies

[6]See Lahno (2007) for a more detailed account of the problem. Different scholars have recognized and analyzed the problem; see Sugden (1991) for an overview on the 'classic' literature. Sugden traces the discovery and first analysis of the problem to Hodgson (1967).

[7]Hindriks (2012) may serve to exemplify this idea. Based on the literature from scholars of the so called "social identity approach" in social psychology he argues that social categorization is a core factor in group identification.

at least one condition of group identification within his account of TR. He, first, refers to theories of group identity and empirical research in psychology to present different states of affairs that may prompt group identification (Bacharach, 2006, 73 ff.). He, then, particularly identifies a source of group identification that is entirely determined by the abstract form of a situation as represented in a game. He refers to this source of group identification as "strong interdependence" (Bacharach, 2006, 84). A situation is one of strong interdependence if there is a feasible outcome that Pareto-dominates every (other) possible solution consistent with the principles of individual rational decision-making as specified by RC. Note that the two abstract situations discussed above, the PD as well as the HiLo, are of this sort. (C, C) Pareto-dominates (D, D), the unique solution according to RC in the PD, and *(red, red)* Pareto-dominates *(blue, blue)* which is the only other possible solution according to RC, i.e., the only other equilibrium, in HiLo.
A rational individual in a situation with strong interdependence will realize that the actors in the situation share a goal which they can collectively achieve. As a consequence, Bacharach claims, the individual will identify with the group. Moreover, if we stick to the common assumption that the rationality of individuals (and the structure of the game) is common knowledge, the fact that individuals identify with the group will also be common knowledge. Although individuals may lack the perfect rationality assumed in theory, similar considerations may well apply to real world individuals. Bacharach, in fact, makes the empirical claim that the perception of strong interdependence stimulates group identification, or more precisely:

The probability of group identification is high if strong interdependence is perceived. $(\star)$

Bacharach argues that reason dictates team reasoning once it is common knowledge that individuals identify with the group. Again, although individuals may lack the perfect rationality assumed in theory, they should be expected to team reason if they identify with the group and perceive others as being alike. This results in another empirical claim, the "reasoning effect" (Bacharach, 2006, 135 ff.). Group identification stimulates team reasoning, or more precisely:

Common group identification induces a high probability of team reasoning. $(\star\star)$

Strong interdependence is particularly obvious and salient in HiLo and it seems natural to assume that real individuals, just as their ideal rational counterparts, are well aware of the fact that there is a shared goal which they can collectively achieve. In the light of this observation $(\star)$ and $(\star\star)$ yield an empirical hypothesis for the HiLo game: there is a high probability of coordination on the Pareto-optimal outcome in HiLo. In other words: TR (as specified by Bacharach) can explain what is commonly observed but cannot be explained within a pure RC approach.

Sugden agrees: TR can indeed provide a valid explanation for coordination in HiLo games. But he rejects the assumption that common knowledge of group identification is sufficient to identify team reasoning as the unique rational mode of reasoning. According to Sugden, it may still be rational to refrain from team reasoning, even if it is common knowledge that every individual involved also identifies with the group. For Sugden team reasoning just remains one rational mode of reasoning among others. Even if it is common knowledge that all individuals identify with the group, it may remain rational (in the very light of group goals) for a member of the group to follow another mode of reasoning, if all others do. So to engage in team reasoning an individual has to be sufficiently assured that others not only identify with the group but also use this mode of reasoning as well.[8]

As a consequence, Sugden demands that any complete explanation by TR has to refer to some form of "assurance". Individuals must have good reason to believe that others identify with the group and employ team reasoning to make their choice. A person that identifies with the group and employs team reasoning as his mode of reasoning is referred to a 'teamer'. For Bacharach, every member of the team is a teamer if group identification (and the rationality of the members) is common knowledge. For Sugden this does not necessarily hold. Individuals must have good reasons to believe that others are teamers, they must have good reasons to believe that the others also have good reasons to believe that group members are teamers, and so on.[9]

But when will this condition be fulfilled? This, again, is an empirical question. Sugden identifies one important source for such assurance: common experience of a shared practice (Gold and Sugden, 2007, 135). If people regularly observe that others act according to the beneficial scheme and if this is a public experience – everybody observes the regularity, everybody is aware that everybody else does, etc., – then each individual is sufficiently assured that sufficiently many others not only identify with the group but also act on the group's goal in the way TR prescribes.

5.4 Instrumental rationality and opportunism

The founders of TR are firmly rooted within the RC tradition. They were motivated by the shortcomings they found within RC, and so they made an attempt to revise RC in ways such that many fundamental ideas of RC are preserved while some of its most pressing problems are solved. Moreover, while team reasoning is understood as genuinely rational, it is not claimed to be the only viable form of rational decision-making. As was noted before, TR has a special focus on interaction in which groups of individuals may

[8]Compare the problem of equilibrium selection in RC as discussed in section 5.2 above!

[9]This is a rough sketch of the condition only. For a precise formulation see Gold and Sugden (2007), 132 ff, and Cubitt and Sugden (2003) for an elucidation of the concept of "reason to believe".

be identified as units of agency. It is not claimed that new insights to issues outside the range of this focus are actually added.

Thus, TR can be understood as an attempt to advance RC by extending the range of possible subjects of agency and generalizing the fundamental concepts of the theory of individual rationality such that they may be applied to contexts where collectives can be understood as agents. As a consequence, TR is fundamentally related to traditional RC. This becomes particularly lucid by the following two observations:

(1) Whatever action TR prescribes to an individual in a certain situation, choosing this action is individually rational in the sense of RC relative to team preferences. Given that the other agents act as teamers the individual maximizes team utility by choosing the act proposed by TR.

(2) RC may be understood as a special case of TR by defining individuals as groups of one. TR then produces the very same theorems as RC on the domain of individuals so defined.

(1) TR is based on a simple insight: a profile of strategies which is proposed by TR maximizes team utility. Hence, there is no other profile with larger team utility and, hence, an individual cannot improve (relative to team utility) by deviating unilaterally. That is, team reasoning is consistent with instrumental rationality in the sense of RC if individual preferences and team preferences are assumed do be identical. However, TR does more than just adding collective goals to RC. This is manifest in the decision rule (tr). As noted above (tr) serves inter alia to solve equilibrium selection problems which cannot be solved by individual instrumental rationality as defined in RC alone.

(2) is a simple consequence of the guiding idea behind TR. Groups of individuals are introduced as additional units of agency. The traditional assumptions of RC about individuals (i.e., that they are characterized by specific preferences and beliefs) and their ways of decision-making are then transcribed to these new units. In this regard, TR is a genuine amplification of RC: it contains RC as a special case. Another consequence is that TR mirrors the key assumptions of individual decision-making in RC in its assumptions about team reasoning.

The essential element of TR's heritage from traditional RC is its rigorous conception of instrumental rationality:

- *According to RC* all individual goals can be represented by a suitable utility function over the consequences of action, and every motivational force can be represented as a disposition to maximize this utility function in a certain choice situation.

  According to TR all team goals can be represented by a suitable team utility function over the consequences of action-profiles, and every collective intention can be represented as a disposition to maximize this utility function in a certain choice situation

- *According to RC* every individual chooses those options that guarantee (relative to his beliefs) maximum individual utility.
  According to TR every team or collective chooses those options that guarantee (relative to common beliefs in the team) maximum team utility.
- *According to RC* any individual will choose opportunistically in the following sense: he will instantly detect every opportunity to improve by changing his way to act and use it immediately; he will do so, whenever the opportunity may arise, i.e., in every single situation of choice. Therefore there is no motivational force in considerations like: what will be the overall consequences of me following this route of action in the long run?
  According to TR any team will choose opportunistically in the following sense: the team will instantly detect every opportunity to improve by changing the profile of actions by the teamers and it will collectively use it immediately; it will do so, whenever the opportunity may arise, i.e., in every single situation of choice. Therefore there is no motivational force in considerations like: what will be the overall consequences of us following this route of action in the long run?

Opportunism[10] combines extreme agility and responsiveness to changing circumstances with resolute restrictions on the determinants of choice. On the one hand, opportunistic decision makers unswervingly take any opportunity as soon as it occurs. An opportunistic individual is not governed by customs or habits. his behavior may conform with norms (if this suits his interests), but it is not restricted by norms. he does whatever is best independently of his own past behavior or of what others may think suitable. On the other hand, there is a clear and rigorous restriction on what may influence the decision of an opportunistic decision maker: only the expected future consequences of own individual choices are relevant for opportunistic choice.
Individual opportunism is both, a distinguishing feature of theoretical clarity and analytical rigor in the architecture of RC, and a major source of RC's problems to cope with the empirical regularities of human decision-making. An important example of the problematic aspect of opportunism in RC is the so-called commitment problem as, e.g., illustrated by the Prisoners' Dilemma. Assume player A could commit herself to cooperate if he expected B to cooperate, and B, after being informed on the commitment of A, could commit herself to cooperate. Then, the two could jointly realize the mutually preferred outcome (C, C). But, although both would benefit from such an arrangement, it is not obtainable to rational individuals in the sense of RC unless they have an external

[10]Cf. Kliemt (2009), 55ff, for the concept of opportunism as used here.

and binding commitment mechanism at their disposal. The sole fact that it is mutually advantageous that both restrict their options does not enable them to actually restrict options, - it cannot be effective as a reason to act. Opportunism is the core reason for this. If the occasion arises each has to choose the optimal option given his beliefs about the actions of the other. As long as a strictly dominant option D is actually available (as in the PD), opportunism commands to choose it. Opportunism makes individuals look solely at the future consequences of their individual choice given their beliefs about other's choices. The fact that regularly following a different scheme of action might be mutually beneficial is irrelevant, as only the choice of each single action is under individual control, but not the choice of a (binding) scheme of action.
HiLo provides another example for the difficulties of opportunistic individual decision-making. That the outcome *(red, red)* is mutually preferred provides no reason for choosing *red* because the outcome *(red, red)* is not an option of an individual. It would provide a reason for an opportunistic individual only if the mutual preferability would somehow justify the expectation that the other would choose *red*. This would make the option *red* optimal. But this is exactly what is at issue. It is the very flexibility of opportunistic choices that prevents individual actors from coordination on a mutually advantageous equilibrium. Since opportunistic actors are perfectly responsive to the conditions of choice and absolutely flexible in their reactions they may loose their ground if the crucial determinant of choice is what they expect from others (as is generally the case in strategic interaction).
Individual opportunism is a theoretically elegant conception but it hardly accounts for the complexities of real life human decision-making. Actual human actors are not perfectly free to instantly detect and choose whatever is best. They are subject to the influence of custom and habit, and they are bound by personal and social norms independently of the support of these norms by sanctions. Such observations may, in fact, explain to some extent why the commitment problem as well as problems of coordination are not as pressing for real life individuals as RC would predict.
TR is an attempt to formulate a theory that offers a remedy against the deficiencies of individual opportunism and, at the same time, accounts for real life restrictions and opportunities in decision-making processes. According to TR, individuals may restrict the range of their options to those part of an optimal scheme by adopting a team perspective. Thus, they may overcome a commitment problem as represented by the Prisoners' Dilemma game. Moreover, they may cut across the endless regress constituted by the mutual dependence of choice and expectation by introducing the social optimum as an authoritative common point of reference. In both cases the neglect of individual opportunism and its consequences allows for a more realistic account of decision-making.
At least in the form fostered by Sugden TR also gives some account of the influence of

custom and habit on decision-making. According to Sugden “assurance” is a necessary condition such that group identification makes individuals act as teamer. The common experience of a shared practice is, as Sugden emphasizes, an important and sufficient condition of such assurance. Hence, according to TR, custom plays an important rule in explaining social interaction. Recurring patterns of behavior in a group are taken as evidence for team reasoning to be the prevalent mode of reasoning in the group and – therefore – justify and motivate team reasoning as the appropriate mode of reasoning. Similarly, norms induce commonly shared patterns of behavior and expectations. If these patterns are consistent with the demands of team reasoning they may bring about a shared perception of team reasoning as the appropriate mode of reasoning. This will in turn consolidate the behavioral pattern.

Notice, however, that this account of the influence of custom and social norms on social interaction is limited by the condition that it is team reasoning instead of customs or norms themselves that induces the regularities of conforming behavior. Custom and norms merely explain the stability of team reasoning as a mode of decision-making. But this stability can only be sustained as far as team reasoning continuously prescribes the same route of action. The stability will break down as soon as a change in the environment implies profitable deviations from a team perspective. As noted above, TR assumes the same kind of opportunism for teams as RC does for individuals. Hence, according to TR, teams display the same sort of rigid flexibility and instant responsiveness to changing circumstances as individuals do according to RC. If a new opportunity arises, the team will detect and take it instantly. Understood as units of agency, teams are assumed to be free of habits, custom or normative restrictions.[11]

There is another, more traditional account of behavioral regularities that also sticks to the fundamental assumption of rational decision-making within RC while extending its explanatory power. On this account social norms are defined as specific equilibria[12] which function as additional constraints on individual decision-making. Thus, norms are understood as the primary source of the regularities and not as a by-product of rational action. Such considerations certainly shed some light on the nature of social interaction in general and on norm guided behavior in particular, but their actual empirical significance is limited. The reason is that – although a general account of the nature of behavioral regularities is given – the theory does not provide an explanation why this or another norm prevails.

[11] And so, the commitment problem and the problem of equilibrium selection may reappear on a higher level if different teams interact strategically.

[12] Strictly speaking, only a specific form of norms (“norms of conduct” can be identified with equilibria, see Lahno (2007, 2010) for a detailed account). Well known representatives of the idea that (some) norms are equilibria are, e.g., David Lewis, Michael Taylor, Robert Sugden, Christina Bicchieri and Ken Binmore (see Gintis, 2010, endnote 1 for references). Gintis (2009, 2010) proposes a generalization of the idea based on the theory of epistemic games and the concept of coordinated equilibrium.

5.5 Changing accustomed patterns of behavior by team reasoning: introducing uncertainty

TR not only offers a general account of how people solve coordination problems, it actually generates testable hypotheses on what regularities are to be expected. In the remaining part of this paper we investigate the empirical significance of TR's assumption of opportunistic decision-making on the team level. We focus on a particular form of change in the environment, which offers opportunities to improve on the team goal by changing an accustomed pattern of behavior. This change consists of the introduction of a particular kind of uncertainty regarding the consequences of individual choice.

Our interest in the impact of uncertainty on team reasoning is originally motivated by Bacharach's provoking and in some respect surprising discussion of the demands of TR in case that there is some (known) probability that others will not conform to TR. This sort of problem is well known from rule-utilitarianism: given that, realistically, a certain fraction of the population will act immorally, what rule should the moral person comply with? Donald Regan (1980) presents "cooperative utilitarianism" as a solution to the problem. This in turn serves Bacharach as a model for the solution of his problem, in his theory of "circumspect team reasoning" (Bacharach, 2006, 130ff; Gold and Sugden, 2007, 131). The theory displays a radical resoluteness in the team perspective ascribed to individual team reasoners by TR, a resoluteness that, in fact, may well arouse some doubts in the empirical significance of TR.

The theory of circumspect team reasoning is interesting in normative respects but a thorough discussion of the theory would go beyond the scope of this paper. We are interested in the extent of opportunism assumed in TR and the empirical plausibility of these assumptions, which is why we discuss the impact of uncertainty on decision-making in TR. The uncertainty in Bacharach's model of circumspect team reasoning is assumed to be endogenous. However, in order to empirically address the impact of uncertainty we consider a related but exogenously defined uncertainty in a very specific and much simpler setting, namely in variants of the HiLo game.[13]

Our point of departure is a HiLo game with payoffs as given in Figure 5.5 (with corresponding parameter a = 1.25 in the representation as in Figure 5.3):

We assume that the given payoffs (the numbers in the bi-matrix) represent standard cardinal utility values, and, as before, we assume that team utilities coincide with individual utilities. Team reasoning then prescribes that every teamer chooses his part in the optimal scheme; accordingly, teamers will coordinate on the Pareto-optimal outcome *(red, red)*.

[13]One major benefit of focussing on exogenously imposed uncertainty is that the latter can be reproduced in the lab, – in contrast to the uncertainty assumed in the theory of circumspect team reasoning. It is hardly possible to exogenously vary the fraction of team reasoners given the same strategic situation in every treatment.

	red		*blue*	
red	5	5	0	0
blue	0	0	4	4

Figure 5.5: Basic HiLo game

We will now, consecutively, introduce two slight modifications of the game. First, we consider the game resulting from the one above, if a random mechanism partly determines the choice of one of the two players, in the following referred to as person A. Depending on the outcome of this mechanism A can either freely choose between *red* and *blue*, with probability ω, or is forced to choose *blue* with probability $1-\omega$. The other player, person B, is not informed on the outcome of this random mechanism. However, the existence of such a mechanism and ω are common knowledge.

Payoffs are again symmetric across players. Figure 5.6 represents the expected outcomes (in terms of team utilities) for the different combinations of strategies. As long as $5\omega > 4 - 4\omega$, i.e., $\omega > \frac{4}{9}$, the game is still a coordination game with equilibria *(red, red)* and *(blue, blue)*. Therefore, both choices *red* and *blue* are rationalizable from an RC point of view. If an individual has reason to expect his partner to choose *red*, choosing *red* is his rational response, and a corresponding statement applies to expecting the partner to choose *blue*: it is a sufficient reason to choose *blue* oneself.

		Player B	
		red	*blue*
Player A	*red*	5ω	$4-4\omega$
	blue	0	4

Figure 5.6: One-sided uncertainty

From a team perspective *(red, red)* is the unique optimal outcome as long as $5\omega > 4$. So TR predicts that teamers will choose *red* in the role of person A as well as in the role of person B if $\omega > 0.8$, and *blue* if $\omega < 0.8$. As TR determines a unique best scheme of action as long as $\omega \neq 0.8$, choices of teamers are uniquely defined.

We conclude: if the likelihood that person A is forced to choose *blue* is high enough (larger than 0.2), then teamers will take the opportunity to maximize expected (team) earnings by changing to *blue*.

The experience of a shared practice in the standard HiLo may assure players that team reasoning is the prevailing mode of reasoning among individuals who identify with the group. But a shared practice owns no independent momentum of inertia, as one might expect from experience with common social practices. TR predicts that *(red, red)* is abundant as soon as *(blue, blue)* becomes optimal from the team's perspective. Yet, *(red, red)* could still serve as a convenient focal point for coordination.

In a second step, we extend this kind of uncertainty to both players. Consider the modified HiLo game in which both individuals are independently confronted with a chance move that might force them to choose *blue*. Two formally identical random mechanisms determine – independently – for each of the individuals whether he can freely make his choice (with probability ω) or is forced to choose *blue* (with probability $1-\omega$). While outcomes of the mechanisms are only communicated to the respective individual but not to his partner, the mechanism as such and the value of ω are again common knowledge. Payoffs remain symmetric. Figure 5.7 represents the expected outcomes for the different combinations of strategies.

This case of two-sided uncertainty is formally identical to the uncertainty that gave rise to Bacharach's introduction of circumspect team reasoning.[14] Hence, the analysis to follow perfectly accords with Bacharach's corresponding discussion (see in particular Bacharach, 2006, 132 f.).

The game in Figure 5.7 is still a coordination game with equilibria *(red, red)* and *(blue, blue)* as long as $5\omega^2 + 4(1-\omega)^2 > 4 - 4\omega$, i.e., as long as $\omega > \frac{4}{9}$. *(red, red)* is the unique optimal outcome as long $5\omega^2 + 4(1-\omega)^2 > 4$. Hence, TR prescribes to choose *red* if $\omega > \frac{8}{9}$ and *blue* if $\omega < \frac{8}{9}$. As with the first modification above: if the likelihood that a partner is committed to choose *blue* is high enough, then team reasoners will take the opportunity to maximize expected (team) earnings by changing to *blue*.

		Player B	
		red	*blue*
Player A	*red*	$5\omega^2 + 4(1-\omega)^2$	$4(1-\omega)$
	blue	$4(1-\omega)$	4

Figure 5.7: Two-sided uncertainty

Hence, if $\frac{4}{5} < \omega < \frac{8}{9}$ team reasoners will coordinate on *(blue, blue)* in case of two-sided uncertainty, while they will coordinate on *(red, red)* in the corresponding case of one-sided uncertainty.

This result may come as a surprise: at first sight the situation of an actor under two-sided uncertainty after being informed that he may freely choose seems to resemble the situation of actor B under one-sided uncertainty in all relevant respects.

Assume $\frac{4}{5} < \omega < \frac{8}{9}$. Consider the situation of an actor A after being informed that he is free to choose. Given this private information the expected utility of the profile *(red, red)*

[14] There is a slight, but relevant substantial difference, though: while there is a known proportion $1-\omega$ of non-teamers in the case of circumspect team reasoning, all individuals in case of two-sided uncertainty as discussed here are assumed to be teamers. In the circumspect team reasoning scenario the non-teamers deliberately choose a default strategy s* (corresponding to *blue* in our case) while in case of two-sided uncertainty teamers are forced to choose *blue*. This difference may be of some relevance in relation to the empirical inclination of individuals to identify with the group. If I know that there is a considerable number of people who do not identify with the group then I might myself be less motivated to identify with the group. So group identification seems to be more precarious in the circumspect team reasoning scenario.

is 5ω. So *(red, red)* is the optimal profile for the team from A's perspective, - it would be best if both choose *red*. Of course, the partner B does not have the same information. B does not know that A is free to choose; therefore it may seem questionable to A that B will do his part in the optimal profile. But notice that, if B actually gets the chance to choose, his situation will be exactly like the one we have just discussed. So B will also find that *(red, red)* is the optimal profile. Finally, from the perspective of any individual free to choose *(red, red)* is the optimal profile; and rational individuals, whether teamers or not, will know this. However, teamers should neglect their private information about their own situation in their decision-making, which is why – seemingly contrary to their interest – TR defines *(blue, blue)* as the optimal profile in case of $\frac{4}{5} < \omega < \frac{8}{9}$. Is it possible, that from the perspective of every player who gets the opportunity to choose *(red, red)* is the optimal scheme of action in terms of group goals, while *(blue, blue)* is in fact optimal? It is! Note that the argument above considers only the expected gains of those that are free to choose. The expected loss of those that are forced to choose *blue* are not taken into account. But in case of $\frac{4}{5} < \omega < \frac{8}{9}$ these losses outweigh the potential gains of those left free to choose. Individual opportunism is at the heart of the strategic calculus unfolded above. It is true: for those that actually can control their choice *(red, red)* is the optimal profile. So if they mutually control the outcome – as TR assumes for the team as a whole – they should choose *red*. Moreover, if all choose accordingly each does his part in the profile that is optimal from his own perspective, - some because they already know that they actually control their choice, others because their choice is irrelevant anyway. But, as in the classical PD: if everybody chooses the best means available to achieve his aims (which, in our case, are actually shared aims) this does not necessarily result in an optimal outcome for each. In fact, the expected outcome for everyone is suboptimal.

TR offers a cure for the detrimental narrow-mindedness of individual opportunism among those that by coincidence acquire the power to decide. Team reasoning yields better results for the team and the individuals on average, by disregarding the individual and concentrating on the collective perspective only. The argument above considers for each actor in a team the case that he is free to choose while it is unknown whether the partner is also free or not. But these cases overlap and they are not exhaustive. To determine the optimal profile for the team one must consider the following four cases which are pairwise disjoint and mutually exhaustive instead:

(i) first player free, second bound to choose *blue*;

(ii) first player bound, second free;

(iii) both players free; and

(iv) both players bound.

Taking all these cases and their respective probabilities into account we get the result stated above: team reasoners will choose *red* if $5\omega^2 + 4(1-\omega)^2 > 4$, and *blue* if $5\omega^2 + 4(1-\omega)^2 < 4$. Team reasoning can, thus, overcome suboptimal choices suggested by individual opportunism to those that effectively control their choices.

However, opportunism remains a crucial characteristic of decision-making according to TR, albeit opportunism concerning team actions in the light of team goals. If the environment changes from $\omega > \frac{8}{9}$ to $\omega < \frac{8}{9}$ teamers will, again, instantly and concurrently change from coordinating on *red* to coordinating on *blue*. A shared practice is entirely volatile under the regime of team reasoning. TR predicts that it will immediately change if the probability of a forced move exceeds $\frac{1}{9}$.

Finally, note the problem of two-sided uncertainty as discussed above may also be framed as a problem of using private information. Individual opportunism demands that every player should use all the information available to his in making his decision. This is largely what drives the argument suggesting that rational players should act in exactly the same way in cases of one-sided and two-sided uncertainty if the parameter ω is identical. But team reasoning demands to refrain from using information not mutually available to all individuals in the team alike. The simple reason is that every teamer must be in the position to determine the team solution to contribute his part, so the solution cannot depend on information that is not available to all. While TR's tendency to transgress individual opportunism may appear as a step towards a more realistic account of social interaction in general, neglecting private information may well seem a too demanding requirement for real human individuals.

5.6 Experimental evidence

5.6.1 Experimental design

We conducted a laboratory experiment to test behavior differences in the situations discussed in the preceding section. The experiment involved three different treatments in a between-subjects design, and each treatment consisting of three parts. The exogenous variation across treatments referred only to the second part of the experiment.

In the beginning of the experiment, every subject was paired with another participant, in the following referred to as his 'team member' or 'partner'. These 'teams' remained the same for the rest of the experiment. In every team one subject was randomly assigned the role of person A, the other subject the role of person B; roles were in fact only effective in Part 2 as described below. Although team members were anonymous throughout, we encouraged team identity by framing pairs as 'teams' in the instructions.[15]

[15]The instructions for treatment ONE-L (described below) are provided in appendix E.1; the instructions for other treatments are available from the authors.

In Part 1, each team played a series of five HiLo games with payoffs (in Euro) as given in Figure 5.5. Feedback about the other team member's choice and the payoff associated with the team's strategy profile was given after every round.[16] By being engaged in a sequence of interactions subjects were induced to identify with their team and to coordinate on one equilibrium outcome. Moreover, by controlling whether team members were able to perfectly coordinate throughout in Part 1, we may assume that group identification as required in TR was satisfied. First, subjects gained experience in the series of HiLos, which we consider sufficient as 'assurance' in the sense of Sugden. Second, the HiLo game displays 'strong interdependence' as is required according to Bacharach.[17]

In Part 2, participants played a one-shot modified HiLo game with uncertainty as defined in either Figure 5.6 or Figure 5.7, depending on the treatment. Treatments differ with respect to the kind of uncertainty and the probability ω. We implemented one-sided uncertainty with $1-\omega=\frac{1}{6}$ (treatment ONE-L(ow risk)) and with $1-\omega=\frac{1}{3}$ (treatment ONE-H(igh risk)), and two-sided uncertainty, again with $1-\omega=\frac{1}{6}$ (treatment TWO-L(ow risk)). Again, the team member's choice and associated payoff was communicated ex-post.

In the ONE-sided treatments, only one team member, namely person A, faced a chance move which forces his to choose *blue* with probability $1-\omega=\frac{1}{6}$ in ONE-L, and with probability $1-\omega=\frac{1}{3}$ in ONE-H. Importantly, person B was not informed on the realization of this chance move at any point in the experiment. In the TWO-sided treatment, both team members faced the risk of being compelled to choose *blue*, with probability $1-\omega=\frac{1}{6}$ and independently of each other. Again, subjects were not informed about the realized outcome of their partner's chance move.[18] As in Part 1 the condition of strong interdependence is satisfied in all treatments, and we assume that assurance gained from the series in Part 1 is still effective.

In Part 3, we elicited individual risk attitudes using choice lists with 16 different decision items (see, e.g., Holt and Laury, 2002). In each of these items participants had to choose between receiving a certain amount of money and participating in a lottery. While the lottery was kept constant across all decisions, the certain amount increased continuously from the first to the last decision item. Finally, at the end of the experiment participants completed a questionnaire on socioeconomic characteristics.

[16]To avoid any income effects, if this part was selected for payoff, only one round was randomly determined to be paid out to subjects. More details on the experimental procedures are provided in section 5.6.2.

[17]Subjects that proved unable to coordinate in Part 1 were excluded from the data set.

[18]To describe the random mechanism in an intuitive way to subjects the realization of chance moves was implemented as follows. In the ONE treatments Person A was assigned an identification number, i.e., an integer ranging from 1 to 6 in ONE-L and ranging from 1 to 3 in ONE-H. Accordingly, in the TWO-sided treatment Person A and Person B were each assigned an identification number between 1 and 6. Then, a virtual dice (6-sided or 3-sided, respectively) was rolled by the computer. In case the dice coincided with the subject's ID, his choice was set to *blue*, while he was free to choose in the other case.

5.6.2 Experimental procedures

Experimental sessions were conducted between June and October 2013 at the Frankfurt School Laboratory, Frankfurt School of Finance and Management, Germany. Participants were recruited from the pool of resident students at Frankfurt School. In total 103 subjects participated in our study.[19] 23.3% were female, the average age was 22 years, and all of them had a business or management background in their studies. Detailed instructions were handed out to participants in the beginning of every part, i.e., not before the preceding part was finished. Instructions were read out aloud by the experimenter in front of participants to exclude the opportunity that participants received private information. Everyone was given enough time to carefully look at them and ask questions before and during the experiment, which were answered in private.

In Part 1, subjects necessarily received feedback about their team members choice and, hence, about the monetary outcome realized by their mutual actions. To avoid any income effects within Part 1 and across Part 1 and 2, for each team, either Part 1 or Part 2 was randomly and equally likely selected for payment. Further, if Part 1 was selected, only one round was again randomly drawn to be payoff-relevant. Additionally, subjects received their earnings from their individual decisions in Part 3. In this part only one of the 16 decision problems was again randomly selected for payoff.[20] If a subject chose lottery A for this particular problem, the computer performed a random draw given the respective distribution and independent from other subjects, to determine the lottery outcome. On top, participants received a show-up fee of € 4.00. On average participants earned € 13.00 (approximately $ 17.50 at the time of the experiment). All of what is written above was common knowledge.

5.6.3 Hypotheses

The condition of strong interdependence is satisfied in every game of Part 1 and Part 2 in the experiment. We further assume that successful coordination on *(red, red)* in Part 1 generates 'assurance' in the sense of Sugden: subjects will have common reason to believe that their partner identifies with their team and makes his decision accordingly. Thus, for those who manage to coordinate in Part 1 TR may predict subjects' decisions in Part 2. Hence, we restrict our analysis to those individuals that continuously succeeded in coordinating on the profile *(red, red)* in Part 1 and assume strong interdependence and assurance to be satisfied in what follows.

If both partners are teamers, then TR predicts coordination on *(red, red)* in ONE-L and coordination on *(blue, blue)* in ONE-H and TWO-L. If team reasoning is in fact the

[19]As the experiment necessitates an even number of subjects, in one session a member of the student assistants staff participated; his data are not included in the analysis.

[20]All random draws were implemented by the computer.

prevalent mode of reasoning in coordination problems, then this results in the following hypotheses for behavior within each treatment in Part 2.[21]

Hypothesis 1 (Strategy profiles).

(H01) A considerable majority of the subjects chooses red in treatment ONE-L.

(H02) A considerable majority of the subjects chooses blue in treatment ONE-H.

(H03) A considerable majority of the subjects chooses blue in treatment TWO-L.

Participants were randomly assigned to treatments, and thus the fraction of teamers and non-teamers may be assumed to be similar across treatments. This yields the following hypotheses for differences across treatments in Part 2:

Hypothesis 2 (Treatment differences).

(H11) The fraction of subjects who choose red in treatment ONE-L is significantly higher compared to treatment ONE-H.

(H12) The fraction of subjects who choose red in treatment ONE-L is significantly higher compared to treatment TWO-L.

(H13) The fraction of subjects who choose red in treatment TWO-L is not significantly different from the fraction of subjects who choose red in treatment ONE-H.

5.6.4 Results

We first comment on behavior in Part 1 before we analyze behavior in Part 2. Results from Part 3, in which we controlled for any effects of risk attitudes, are addressed within the discussion in section 5.6.5.

Part 1

Although RC does not imply a prediction for this series, it is not surprising that all but one team consistently coordinated on the strategy profile *(red, red)*. For the reasons explained above, the data of those two subjects who failed to coordinate is excluded from the analysis of Part 2. This leaves us with 101 observations, 36 observations in treatment ONE-L (18 subjects in the role of person A, 18 in the role of person B), 45 observations in treatment ONE-H (23 in role A, 22 in role B), and 20 observations in TWO-L (role A and role B were perfectly congruent).

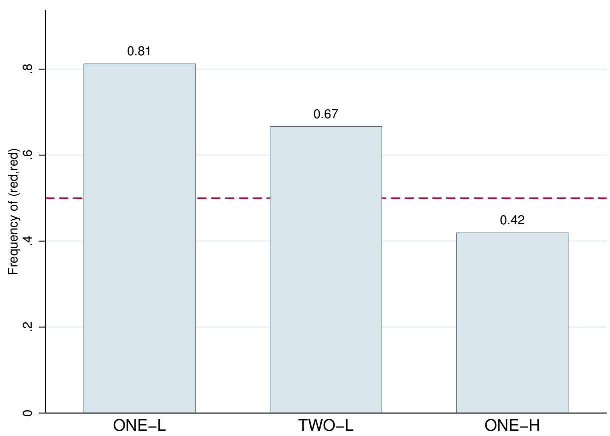
[21] In our theoretical discussion of the HiLo game (with and without uncertainty) we assumed expected utility theory with near-linear utility functions. Using the data from Part 3 we controlled for this conditions as outlined in section 5.6.5.

Part 2

In ONE-L, two A players were forced to choose *blue* in Part 2; in ONE-H, eight A players were forced to choose *blue*; in TWO-1/3, two subjects were forced to choose *blue*. All numbers provided in the following analysis refer only to those subjects that could actually make a choice, i.e., we do not count enforced choices.

Strategy profiles

TR predicts that in ONE-L teamers should choose *red*, and we in fact observe that a significant majority chooses accordingly (two-sided-binomial test; p-values 0.001, 0.035, 0.013, for the pooled sample, for A and B players, respectively).[22] Figure 5.8 displays the fraction of subjects who chose *red* for each treatment. While more than 81% chose *red* in ONE-L, this fraction shrinks to 67% in TWO-L, and to 42% in ONE-H. This clearly supports Hypothesis (H01). Consistent with TR, we also observe a lower frequency of *red* choices in both other treatments. While in ONE-H a majority of subjects chose *blue*, in support of (H02), a majority of subjects still chose *red* in TWO-L, contrary to (H03). However, neither (H02) is sufficiently supported nor can (H03) be rejected by the data: we cannot reject the hypothesis that the fraction of *red* choices is statistically significantly different to 50% (two-sided binomial tests; p-values 0.238, 0.508, 0.508 in TWO-L, and 0.473, 1.0, 0.238 in ONE-H, again for the pooled sample, for A and B players, respectively).



Notes: numbers only refer to choices of subjects who were eligible to make a choice, i.e., enforced decisions of A players in ONE-H and ONE-L, and of A or B players in TWO-L are excluded.

Figure 5.8: Frequency of *red* choices by treatment

In TWO-L, we can at least reject the hypothesis that the average fraction of *red* choices

[22]The statistical analysis is conducted in the most conservative way, treating observations of team members not as independent observations, given that team members interacted in Part 1. However, given that we drop those observations where team members failed to coordinate throughout in Part 1, and given that participants and, in particular, team members stayed anonymous during the experiment, we may have also treated team observations as independent, in which case our results remain unchanged.

in TWO-L is smaller than 0.48 (p-value 0.088, one-sided binomial test). This yields evidence against the prediction that a considerable majority generally chooses *blue*, i.e., against (H03). Overall, given the limited number of observations, we find some evidence against (H03) in TWO-L, and some evidence for (H02) in ONE-H.

Strategy profiles by role

According to TR team members should coordinate on the optimal strategy profile which is symmetric in roles. That is, predictions (H01), (H02) and (H03) do not depend on whether players are assigned role A or B, i.e., face the uncertainty of being forced to choose *blue* themselves. Hence, in a next step, we differentiate by role. Table 5.1 provides the fraction of subjects who chose *red*, by role and treatment.

Treatment	Player B	Player A	Player A & B	N
ONE-L	82.35 %	80.00 %	81.25 %	34
TWO-L	66.67 %	66.67 %	66.67 %	20
ONE-H	33.33 %	53.85 %	41.94 %	31

Notes: enforced decisions again are excluded.

Table 5.1: Fractions of *red* choices by treatment and role

Both, with low risk of *blue* enforcement in ONE-L and TWO-L, role does not significantly matter. In contrast, we observe a striking difference between choices of A players and B players in ONE-H. The overall effect in this treatment – i.e., that a majority chose *blue*, which is in line with (H02) – is apparently driven by the behavior of B players. While 67% of them chose *blue* only 46% of A players chose *blue*, contrary to what TR predicts.[23] For B players we can again reject the hypothesis that the fraction of subjects who chose *blue* is smaller than 52% (p-value 0.088, one-sided binomial test), providing mild support for (H02) and B players. However, there is considerable doubt that (H02) also applies to role A players.

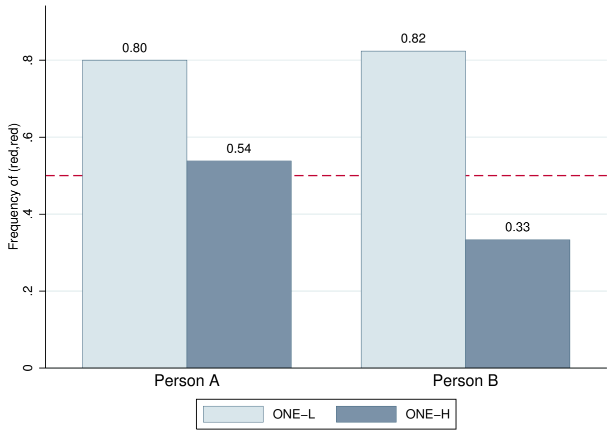
Result 1. *We find significant evidence for (H01); we do not find significant evidence for (H03) and only mild support for (H02). We find evidence for (H02) only for B players and not for A players, which is not consistent with TR.*

Treatment differences

According to TR the distribution of choices should also differ between treatments. First, we find significant evidence for (H11): the frequency of *red* choices is significantly different between ONE-H and ONE-L (χ^2- and Fisher exact test, p-values 0.006 and 0.005, respectively, collapsing over teams). However, if we distinguish between player A and

[23]Using a χ^2-test the difference between the fraction of *red* choices is not significantly different between players A and B, p-value 0.253. Yet, our sample only counts 13 A players and 18 B players.

player B (see Figure 5.9) we again find that this difference is significant only for B players (p-values 0.003 for B, 0.139 for A). This finding is not consistent with TR, since theory does not predict different effects for different roles.



Notes: enforced decisions again are excluded.

Figure 5.9: Treatment effects for one-sided uncertainty by role

Second, TR would predict a significant drop of *red* choices from ONE-L to TWO-L (H12). We do, in fact, observe a moderate drop of *red* choices, but this change is not significant at a satisfying level (p-values 0.351 and 0.315, respectively, collapsing over teams).
Lastly, our observations are neither consistent with (H13). While theory does not predict a difference between ONE-H and TWO-L (H13), we only find that the frequency of *red* choices is marginally significantly lower in ONE-H compared to TWO-L (p-values 0.130 and 0.131, respectively, collapsing over teams). But again this result is driven by role B behavior (p-value 0.010). The behavior of A players is not significantly different in the two treatments (p-value 0.548). Although the latter might be consistent with theory, the fact that the majority of A players choose *red*, as noted above, is not. Overall, the observed treatment differences again do not support TR's predictions:

Result 2. *We find significant evidence for (H11); we do not find significant evidence for (H12) and (H13). Further, (H11) is only supported by behavior of B players, but not of A players.*

Coordination Rates

We briefly comment on coordination rates and realized profits. Surprisingly, in both treatments with one-sided uncertainty, the fraction of teams who managed to coordinate on either *(red, red)* or *(blue, blue)* is quite high - and similar. While 64.7% coordinate in ONE-L, even 67.6% coordinate in ONE-H where the probability that player A is forced to choose *blue* is even higher. Under two-sided uncertainty, only 40% succeed to coordinate. However, in ONE-H, of those who manage to coordinate, only 28.0% ultimately coordinate on *(red, red)*, but 72% on *(blue, blue)*. In contrast, 90.9% conditionally coordinate on *(red, red)* in ONE-L, and at least 75% in TWO-L, under two-sided uncertainty. Under one-sided uncertainty, χ^2-tests show that successes in conditionally coordinating on *(red, red)* are significantly less frequent under high compared to low uncertainty (p-value 0.001, collapsing over teams). It is also significantly smaller in ONE-H compared to two-sided uncertainty (p-value 0.074). Put differently, conditionally coordinating on *(blue, blue)* is significantly more frequent in ONE-H compared to both other treatments. Finally, these coordination rates naturally translate into average earnings which are highest in ONE-L and lowest in TWO-L.

5.6.5 Discussion

Result 1 and Result 2 reveal that we do not find good evidence for opportunistic decision-making according to TR. Consistent with TR's prediction (H01) we observe that a significant majority of subjects choose *red* under the condition that only player A faces the uncertainty of being forced to choose *blue* and that the probability of *blue* enforcement is rather low (1/6). Also consistent with TR's prediction (H02) we observe that a majority of subjects choose *blue* under one-sided uncertainty if the probability of *blue* enforcement is rather high (1/3).

However, if we discriminate for roles A players and B players appear to behave differently in the asymmetric situation of one-sided uncertainty with a high risk of player B being forced to choose *blue*. In ONE-H the majority of A players still choose *red* instead of *blue*, i.e., not consistently with TR's prediction. This result makes us doubt whether the initial evidence for (H01) actually supports TR. The behavior under two-sided uncertainty provides further arguments against TR. The majority of subjects chose *red* in TWO-L, contrary to the prediction (H03). One possible argument might be that a subject who learned that he is free to make a choice, might perceive the risk that the other is forced to choose *blue* to be rather low (1/6). In this way, he would indeed make use of his private information which is not available to his group member.

Treatment comparisons also yield mixed results with respect to TR's predictions. While (H11) seems to be supported at a first glance, distinguishing between roles again shows that (H11) is supported for B players only. Finally, we observe a significant difference between ONE-H and TWO-L and, thus have to reject (H13), which in turn contradicts TR. Apart from the weak support for TR when averaging over both roles, the differences in behavior and treatment effects across roles is generally inconsistent with TR.

Alternative theories of choice and the importance of private information

The only solid support in favor of TR seems to be the significant evidence for (H01). However, this result is also consistent with other theories of choice. In particular, a simple variant of cognitive hierarchy theory (Camerer et al., 2004) with custom as an input delivers the same prediction, assuming sufficient similarity between choice situations in Part 1 and Part 2:

(0) Assume that level 0 individuals tend to stick to an accustomed pattern of behavior if choice situations are sufficiently similar. In our set up, after successful coordination in Part 1, level 0 individuals should choose *red* in Part 2.

The same remains true if strategic reasoning is added:

(1) Assume that level 1 individuals believe that others act in line with (0) and maximize utility given this belief. If the fraction of level 0 players that choose *red* is sufficiently high, it is rational for level 1 players to choose *red* as well.

(2) Assume that level 2 individuals believe that a certain fraction of the others act as level 0 players while others act in line with (1). And assume, again, that level 2 players maximize utility given this belief. In this case it is, again, rational for them to choose *red*.

Obviously, for every higher level of strategic reasoning we get the very same result. So the assumption that our population is composed of subjects using different levels of strategic reasoning in the sense of cognitive hierarchy theory combined with the assumption that custom produces predominantly *red* choices among level 0 players yields hypothesis (H01): a considerable majority will choose *red*. What we learn from these considerations is: the alleged support for TR by the confirmation of (H01) in treatment ONE-L is quite weak. TR does not explain our observations better than the assumption that behavior is simply determined by habit or custom or a theory that combines such an assumption with some strategic reasoning of any level as in cognitive hierarchy theory.

Moreover, this simple cognitive hierarchy model is not only on a par with TR in explaining our observations under one-sided uncertainty, in contrast to TR it also offers an explanation of our observation that a majority of the subjects chose *red* under two-sided uncertainty:

(0) Assume that level 0 individuals tend to stick to an accustomed pattern of behavior if choice situations are sufficiently similar. In our case, after successful coordination in Part 1, a majority of level 0 individuals choose *red* in Part 2.

Again, this remains true under strategic reasoning:

(1) Assume that level 1 individuals believe that others act in line with (0) and maximize utility given this belief. Given our assumptions for level 0 it is rational for them to choose *red*, if the probability p that level 0 players choose *red* and the probability ω that they are free to choose are sufficiently high.

Obviously the same applies to higher level strategic reasoning. Therefore cognitive hierarchy with an input of (sufficiently effective) custom on level 0 produces a prediction consistent with our observation.

A particularly striking observation is the asymmetry between role A and role B behavior in ONE-H. Remember that the game subjects are playing in ONE-H is given by a symmetric strategic form with symmetric payoff outcomes - although only one team member faces the risk of not being allowed to choose. Whatever strategy is optimal for A must also be optimal for B. If there is a significant difference between the choices of role A and role B players this must be due to an asymmetry in the interaction that is not captured in the strategic form. Such an asymmetry arises in the course of the game when player A obtains private information on whether he can make his choice or not. In TR this asymmetry is not taken into account. In the simple cognitive hierarchy model it is. In fact, because cognitive hierarchy accounts for this asymmetry it also provides an explanation of the asymmetry in behavior that we observe in ONE-H:

(0) Assume that level 0 individuals tend to stick to an accustomed pattern of behavior if choice situations are sufficiently similar. In our case, after successful coordination in part 1, level 0 individuals choose *red* with some probability p significantly larger than 1/2.

Assuming sufficient similarity between choice situations in Part 1 and Part 2 of ONE-H custom would produce a majority of *red* choices. The asymmetry gets in, when strategic reasoning is considered in higher levels:

(1) Assume that level 1 individuals believe that others act in line with (0). Consider a level 1 player in role A maximizing utility given this belief. If $p > \frac{4}{9}$ his optimal choice is *red*. In contrast a level 1 B player faces a *blue* choice of his partner with probability $1 - p \cdot \frac{2}{3}$. If $p < \frac{2}{3}$ his optimal choice is *blue*. Thus, for $\frac{4}{9} < p < \frac{2}{3}$, we get: level 1 A players will choose *red*, level 2 B players will choose *blue*.

This pattern is reproduced for higher levels. A players of level n will choose *red* as long as they believe that the overall fraction of B players of lower levels who choose *red* is larger than $\frac{4}{9}$. B players will choose *blue* as long as they believe that the overall fraction of A players choosing *red* is smaller than $\frac{2}{3}$.

If the population is suitably composed of actors of different reasoning levels (e.g. composed of all levels with the fraction of level 0 players being sufficiently high and the fraction of level 1 players being comparably low) this will create a pattern as the one we observe: a majority of A players choose *red*, while a majority of the B players choose *blue*.

We are not claiming that cognitive hierarchy theory offers the correct and unique explanation for the behavior in our experiment. However, considering cognitive hierarchy theory as a theory of choice sheds some light on why TR seems to fail as an explanation. In particular, our discussion highlights the two main weaknesses of TR as an empirical theory of choice:

First, bygones are bygones. As a theory of opportunistic choice TR cannot account for the influence of past behavior on future choice. A regularity observed in the past can inform individuals on the type of players they are paired with - in Sugden's version of TR it may inform us on team identification. But it has no direct impact on the behavior to be expected. Therefore TR cannot account for habitual and customary behavior and its indubitable impact on social interaction. Second, TR confines the individual decision maker to a team perspective that can be shared by everyone. This severely restricts the use of arguments based on private information gained in the course of the game to determine optimal choice. In the experiment A players appear to use their private information on their realized outcome, although TR prohibits them to do so.

Risk attitudes

So far, in theory as well as empirically, we neglected the potential influence of risk attitudes, we simply assumed near-linear utility functions. Risk aversion which is predominantly found among subjects in experiments does not have a significant effect on the main predictions of TR. We still elaborate on possible confounds at this point.

From a TR perspective, in Part 2 of the experiment subjects choose among two lotteries given by the payoffs of the two equilibrium outcomes.[24] In all treatments group members gain 4 currency units if both coordinate on *(blue, blue)*.

In ONE-L by coordinating on *(red, red)*, subjects enter a lottery that yields 0 with probability $1 - \omega = \frac{1}{6}$, and 5 otherwise. Risk neutral and risk seeking team reasoners as well as moderately risk averse individuals are predicted to choose *red* (as assumed in (H01)). In contrast, if individuals are sufficiently risk averse, they should rather go for *blue*. Thus, taking risk aversion into account might result in a slight variation of Hypothesis (H01). This was in fact the only hypothesis that is actually supported by our data with satisfying significance.

In ONE-H, coordinating on *(red, red)* implies the lottery that yields 0 with probability $1 - \omega = \frac{1}{3}$ and 5 otherwise. In TWO-L by coordinating on *(red, red)* individuals enter

[24]To make the following argument we assume that individuals simply transcribe their individual utility function to the team, and thus, under expected utility theory, transfer their individual risk attitude onto the group.

a lottery that pays 4 with probability $(1-\omega)^2 = \frac{1}{36}$, 5 with probability $\omega^2 = \frac{25}{36}$, and 0 otherwise. In both treatments (since expected values are smaller than 4) risk neutral and risk averse team reasoners as well as moderately risk seeking individuals are predicted to choose *blue* (as assumed in (H02) and (H03)). Only sufficiently risk seeking individuals should go for *red*. If subjects are predominantly risk neutral or risk averse (H02) remains unaffected by taking risk attitudes into account.

Hence, in ONE-L sufficiently risk averse teamers might choose *blue*, while in ONE-H and TWO-L sufficiently risk seeking teamers might choose *red*. Assuming that risk aversion predominates risk seeking we get a slight weakening of (H11) and (H12): some risk averse individuals are predicted to choose *blue* in ONE-L as they do in ONE-H and TWO-L, thus weakening the difference between treatments ONE-L and ONE-H and TWO-L, respectively.

Comparing ONE-H and TWO-L as in (H13) and assuming that risk aversion predominates risk seeking we get only a very a slight weakening of (H23) because team reasoners will only act differently in the two treatments if they are risk seeking in a quite peculiar way. They must evaluate '4 for sure' better than the lottery that delivers 0 with probability $1-\omega = \frac{1}{3}$ and 5 otherwise, but worse as the lottery that delivers 4 with probability $(1-\omega)^2 = \frac{1}{36}$, 5 with probability $\omega^2 = \frac{25}{36}$ and 0 else. Such an individual will – according to TR – choose *red* in TWO-L but *blue* in ONE-H. So, to the extent that such individuals are among the subjects, TR predicts a weakening of (H23). These subjects will behave differently in the two treatments.

The sample size of our experiment is quite limited in order to identify any of the above effects. Nevertheless, we collected data about subject's risk attitudes to test whether any behavioral changes that might support or undermine TR are due to risk rather than to team reasoning. In Part 3, we included a choice list to elicit risk attitudes w.r.t. a risky lottery related to the strategic (risky) situation in the experiment. In 16 decisions subjects had to choose between certain amounts of Euros (0.5; 1; 1.5; 2; 2.5; 3; 3.2; 3.4; 3.6; 3.8; 4; 4.2; 4.4; 4.6; 4.8; 5) and a risky lottery that pays € 0.00 with probability 1/6, and € 5.00 with probability 5/6. In ONE-H subjects completed an additional list in which the lottery pays € 0.00 with probability 1/3, and € 5.00 with probability 2/3. The switching point at which a subject switches from choosing the lottery to choosing the certain amount defines the individual's certainty equivalent (CE), i.e., the smallest amount preferred to the lottery.[25] We define subjects as risk averse if their certainty equivalent is smaller than the expected value of the respective lottery.

We find that at least 80% of subjects are risk averse (80.65% in ONE-H, 84.4% in ONE-L and 94.5% in TWO-L), in line with the assumption made above. However, we cannot

[25] Additionally, in the questionnaire conducted at the very end of the experiment we asked subjects to assess their risk attitude on a scale from 1 to 10, however, this was not incentivized.

identify any of the relationships between risk aversion/seeking and the likelihood to choose *red*, which we considered. Risk averse and risk seeking individuals are nearly equally likely to choose *red*, see Table E.1 in appendix E.2 for details on choices by treatment, role and risk attitudes.

5.7 Conclusion

We do not find good evidence for opportunistic team reasoning as a guide to coordination in our experiments. Our observations suggest that individuals tend to stick to behavioral patterns they are in some way or other accustomed to. As a theory of opportunistic choice TR categorically contradicts such influence of past behavior on future choice. Moreover, we find significant differences in the behavior of subjects in accordance with their individual and particular situation of choice. Individuals may obviously take everything into consideration that they get to know about their individual situation in the course of the interaction. In contrast, TR demands to determine optimal play entirely from a team perspective that is equally accessible to all members of the team.

The extent of evidence against TR may surprise in the light of the intuitive plausibility that team reasoning seems to have. In fact, in informal conversations with and among subjects after the experiment we often heard arguments that sounded as if they were directly taken from a course in team reasoning: "The best thing we could do was ... so this is what each of us did!"; "My partner did not understand the problem, we should have chosen ... but he did ..." etc.

We have no doubt that TR would earn overwhelming approval by most individuals if introduced as as a normative theory of choice. Moreover, our intuition as well as the anecdotical evidence from discussions about the experiments suggest that there is also some descriptive truth in TR. Team reasoning as a reasoning procedure just seems too familiar to all who get acquainted with the theory.

Our hunch is that the failure of TR in our experiments is not so much due to empirical inadequateness of the team reasoning procedure in principle, but rather a consequence of the specific conditions of choice. Bacharach assumes, that individuals will engage in team reasoning as soon as they take on the team perspective, i.e., as soon as they identify with the team. Sugden adds another necessary condition: individuals need some mutual assurance that others reason in the same way. These conditions still seem to be quite weak and are easily satisfied in our experimental setting. If our observations show that team reasoning as conceptualized in TR does not in fact determine choice to the postulated extent, this may well point to a misapprehension of the conditions that trigger team reasoning rather than to a fundamental misconception in the idea of team reasoning as such.

Still, there might be a more fundamental shortcoming in the background. In TR team reasoning is understood as a procedure of purely individual reasoning. The team is understood as a collective agent, but not as the subject of collective reasoning. So the 'team' in 'team reasoning' points to the object rather than the subject of reasoning. The reasoning process is located entirely in each individual's mind.

The reluctance to ascribe reasoning processes to collectives is possibly grounded in the fact that collectives have no brains or minds beyond the brains and minds of their members. But, of course they also cannot act beyond what can be done by the concert of their members acts; and we still ascribe agency to them. Collective reasoning processes may be hard to define in theory, but they are not mysterious at all. We all know them in practice and we all know how to participate in them. Maybe this is the very reason that team reasoning appears so familiar to us.

However, collective reasoning processes in this sense may well require extended communication among the team members – which was carefully precluded in our experiments. How would TR have performed in our experiments, had we allowed partners to discuss things out before playing the game?

Appendix A

Peer Effects in Risk Taking

A.1 Theoretical framework

A.1.1 A model of relative payoff concerns

Assume the utility in state j ($j \in \{g, b\}$) of having chosen lottery i ($i \in \{A, B\}$) and earning m_i^j, to be given by the sum of two terms: a consumption utility, which is solely determined by individual risk preferences, plus a social utility term, which depends on payoff differences. This implies $v_{i,k}^j = u(m_i^j) + R(m_i^j - m_k^j)$, where $k \in \{A, B\}$ is the lottery of the peer, and $R(\cdot)$ is a function of payoff differences and defined as follows:

$$R(x) = \begin{cases} x & \text{if } x \geq 0, \\ \lambda x & \text{if } x < 0. \end{cases}$$

The parameter λ captures how large losses with respect to the peer loom relative to gains. An individual's expected utility from choosing lottery i is

$$V_{i,k} = U_i + \sum_j p_j R(m_i^j - m_k^j),$$

where U_i is the expected consumption utility of lottery i. If the peer holds a lottery that yields a lower consumption utility, the individual may nevertheless choose it, if he experiences a strong disutility from falling behind the peer, i.e. if λ is large enough. Let us define an individual's strategy space as $\mathcal{S} = \{\text{imitate} = (i; AA, BB), \text{ deviate} = (i; BA, AB), \text{ stay} = (i; iA, iB), \text{ change} = (i; -iA, -iB); \text{ for } i \in \{A, B\}\}$. Here i ($-i$) denotes his (opposite) choice in Part I, and the tuple ik describes the choice of lottery i in Part II given that his peer has lottery k. Then, the cutoffs are given by the following proposition.

Proposition 1. *Define* $\Delta \equiv \frac{U_B - U_A}{p\delta} + \frac{(1-p)(c-\delta)}{p\delta}$ *and* $\Theta \equiv \frac{U_A - U_B}{(1-p)(c-\delta)} + \frac{p\delta}{(1-p)(c-\delta)}$.

An individual imitates if $\lambda > \max\{\Delta,\ \Theta\}$. *An individual deviates if* $\lambda < \min\{\Theta,\ \Delta\}$. *An individual stays with his Part I choice otherwise.*

Note that whether Δ is smaller or greater than Θ is determined by the individual's choice in Part I, i.e. by his expected consumption utility U_A and U_B.

Proof. An individual imitates if $V_{A,A} > V_{B,A}$ and $V_{B,B} > V_{A,B}$. $V_{B,B} > V_{A,B}$ is equivalent to

$$\lambda(1-p)(c-\delta) > U_A - U_B + p\delta \quad \Leftrightarrow \quad \lambda > \Delta \equiv \frac{U_A - U_B}{(1-p)(c-\delta)} + \frac{p\delta}{(1-p)(c-\delta)}.$$

$V_{A,A} > V_{B,A}$ is equivalent to

$$\lambda p\delta > U_B - U_A + (1-p)(c-\delta) \quad \Leftrightarrow \quad \lambda > \Theta \equiv \frac{U_B - U_A}{p\delta} + \frac{(1-p)(c-\delta)}{p\delta}.$$

Hence, for an individual to imitate it must hold that $\lambda > \max\{\Delta,\ \Theta\}$.
Similarly, an individual deviates if $V_{A,A} < V_{B,A}$ and $V_{B,B} < V_{A,B}$. It follows directly from above that this is satisfied if $\lambda < \min\{\Delta,\ \Theta\}$.

A.1.2 Choice-dependent relative payoff concerns

Assume relative payoff concerns to be defined by the comparison term $R(\cdot)$ as defined in A.1. If λ increases, the likelihood of imitation increases. At the same time, the weight on the payoff differences between lotteries A and B increases. Specifically, the disutility from falling behind when choosing A would increase by $(1-p)(c-\delta) = (1-p)c(1-(1-p)f)$. The disutility from falling behind when choosing B would increase by $p(20-(20-\delta)) = p(1-p)cf$. If the expected value of A equals that of B ($EV_A = EV_B$), or equivalently $f = 1$, the increase is of the same magnitude. However, if $f > (<)1$, then $1-(1-p)f < (>)pf$, and the increase is stronger in magnitude for the case the individual chooses B (A).
Assume that – instead of λ – the social comparison term $R(\cdot)$ is increased by a factor α. Then, the marginal change in social utility from α is given by

$$\begin{aligned} & -(1-p)(c-\delta)\lambda + p\delta \ \text{ when choosing } A, \\ \text{and by} \quad & -p\delta\lambda + (1-p)(c-\delta) \ \text{ when choosing } B. \end{aligned}$$

Hence, the change in utility is smaller when choosing A than B, i.e. the incentive to imitate B is larger compared to A, if

$$\begin{aligned}
& -(1-p)(c-\delta)\lambda + p\delta < -p\delta\lambda + (1-p)(c-\delta) \\
\Leftrightarrow \quad & \lambda\,[p\delta - (1-p)(c-\delta)] < (1-p)(c-\delta) - p\delta \\
\Leftrightarrow \quad & \begin{cases} \lambda & < -1 \quad \text{if} \quad p\delta - (1-p)(c-\delta) > 0; \\ \lambda & > -1 \quad \text{if} \quad p\delta - (1-p)(c-\delta) < 0. \end{cases}
\end{aligned}$$

In line with the seminal model of Fehr and Schmidt (1999) we would assume $\lambda \geq 1$. Following from the same argument as above, i.e. $p\delta - (1-p)(c-\delta) < 0 \quad \Leftrightarrow \quad f < 1$, we would expect more imitation towards B if $f < 1$. Similarly, we would expect more imitation towards A if $f > 1$. This is consistent with Hypothesis 1A.
Lastly, assume that the social comparison term is given by $\tilde{R}(x) = \mu \cdot x \cdot \max\{x; 0\} + \lambda \cdot x \cdot \min\{x; 0\}$. If both μ and λ increase by factor α, the incentives to imitate A or B do depend on how λ relates to μ. Specifically, the marginal change in social utility when choosing A is smaller than that when choosing B if

$$\begin{aligned}
& -(1-p)(c-\delta)\lambda + p\delta\mu < -p\delta\lambda + (1-p)(c-\delta)\mu \\
\Leftrightarrow \quad & \lambda\,[p\delta - (1-p)(c-\delta)] < \mu\,[(1-p)(c-\delta) - p\delta] \\
\Leftrightarrow \quad & \begin{cases} \lambda & < -\mu \quad \text{if} \quad p\delta - (1-p)(c-\delta) > 0 \quad (\Leftrightarrow \quad f > 1)\,; \\ \lambda & > -\mu \quad \text{if} \quad p\delta - (1-p)(c-\delta) < 0 \quad (\Leftrightarrow \quad f < 1)\,. \end{cases}
\end{aligned}$$

If we assume that $\mu \geq 0$, i.e. individual gain from being better off, then $0 < \lambda < -\mu$ is clearly a contradiction. Hence, if $f < 1$ we would again expect more imitation towards B, and if $f > 1$ more imitation towards A. On the other hand, if we allow $\mu < 0$, i.e. allow for the possibility that social gains enter negatively into social utility, then our predictions from Hypothesis 1A only hold if $\lambda > -\mu = |\mu|$ is satisfied. This assumption was already placed in Fehr and Schmidt (1999), i.e. "a player suffers more from inequality that is to his disadvantage" (Fehr and Schmidt, 1999; p. 823).

A.1.3 A model based on social comparison theory

Consider a model in which, the closer the individual risky choice is to the social anchor, the more utility the individual derives. In a setting with only two options, this can be captured by an additional utility γ when the option chosen coincides with the social anchor. In particular, the expected utility of lottery i given the anchor k is

$$V_{i,k} = U_i + \gamma \cdot \mathbf{1}(i = k),$$

where $\mathbf{1}(\cdot)$ is the indicator function. (Cooper and Rege (2011) also assume this form of utility when examining conformism.) Based on the argument above, we would expect γ to differ across treatments and γ_C, in Choice, to be larger than γ_R, in Rand. This would generate an increase in imitation in Choice. Further, since the effect of γ is independent of lottery characteristics, we would expect the change in imitation across treatments to be symmetric with respect to the two available options, A or B.

A.2 Instructions for the Choice treatment

Welcome to the experiment.
Thank you very much for participating. Please refrain from talking to any other participants until the experiment is finished.

General information on the procedure
The purpose of this experiment is the analysis of economic decision-making. During the course of the experiment you can earn money which will be paid out to you at the end of the experiment. The experiment lasts about 1 hour and consists of two parts. At the beginning of each part you receive detailed instructions. If you have questions after the instructions or during the experiment please raise your hand. One of the experimenters will come to your place and answer your questions in private.
While you take your decisions a small clock will count down at the upper right corner of your computer screen. This clock serves as an orientation for how much time you should need to take your decision. However, the countdown will not be enforced in the case that you need more time to come to a decision. Especially in the beginning you might need more time.

Payment
In both parts of the experiment your income is directly calculated in Euro. This amount will be paid out to you at the end of the experiment. For your punctual arrival you receive an additional 4 euro.

Anonymity
The experimental data will only be analyzed in the aggregate. Names will never be connected with the data from the experiment. At the end of the experiment you have to sign a receipt, confirming that you received your payoff. This receipt only serves our sponsor's accounting purposes. The sponsor does not receive any further data from the experiment.

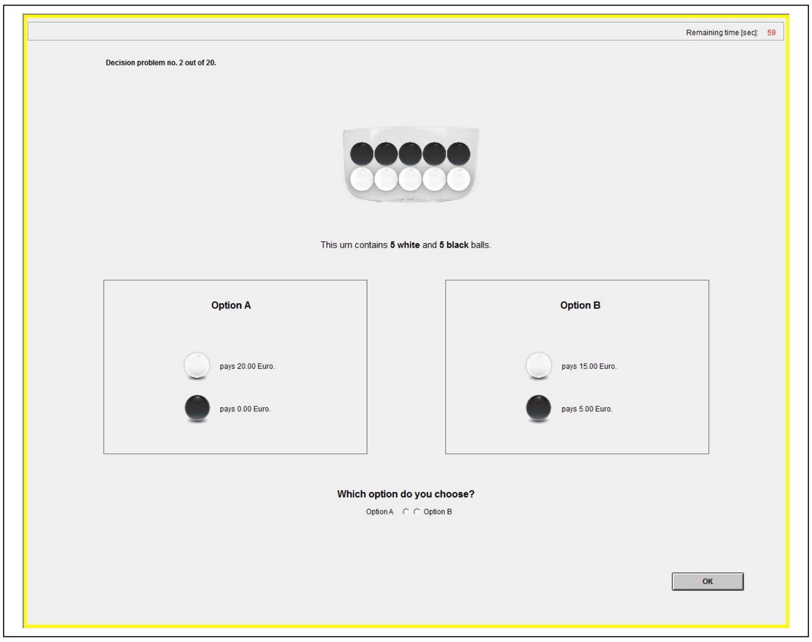
Devices
At your place you find a pen. Please leave the pen at your place at the end of the experiment.

Part I

Task

You will be presented 20 decision situations. In every situation you can choose between two options, option A and option B. Consider your choice carefully, as your choice can - as described below - affect your payoff.

On the screen your will be shown one or two urns which contain white and black balls. The screen will further inform you about the number of white balls and the number of black balls in each urn. Furthermore you will be informed about the value of each white ball and the value of each black ball, in the case that you choose option A or option B, respectively. From each urn one ball will be randomly drawn. If there is only one urn the ball which was drawn is relevant for both options, A and B. If there are two urns the ball will be drawn from the urn which belongs to your chosen option. This is how your screen might look like.



Example - Decision Problem

In this example there is only one urn which contains 10 balls: 5 white balls and 5 black balls, i.e. the probability that a white ball is drawn amounts to 50

Should a white ball be drawn from the urn you receive 20 Euro if you chose option A or 15 Euro if you chose option B. If a black ball is drawn from the urn you receive 0 Euro if you chose option A or 5 Euro if you chose Option B.

The urns in the 20 decision situations are always filled according to one of the following types:

- Type 1: 5 white balls and 5 black balls
- Type 2: 8 white balls and 2 black balls
- Type 3: 2 white balls and 8 black balls
- Type 4: 2 white balls and 6 black balls

You take your decision by marking either option A or option B on the screen. Your decision is final once you clicked the OK-button in the lower part of the screen. In addition to these instructions you are given a sheet of paper on which all decision situations are printed out. Please note on this paper which decisions you have taken.

Payoff

At the end of part II of this experiment one participant will be chosen randomly by the computer. This participant will be assigned the role of an assistant. You will be shown on your screen whether you have been assigned this role or not. The assistant will help the experimenter to randomly determine which part and which decision situations are payoff-relevant.

For this purpose the assistant will first draw one ball out of a nontransparent bag which contains 2 balls - marked with the numbers 1 and 2. This ball decides whether part I or part II of the experiment is payoff-relevant for all participants. The experimenter will type in this number at the assistant's computer.

Assume that part I is drawn as being payoff-relevant. Then, for each participant, the assistant draws one ball out of a nontransparent bag which contains 20 balls numbered from 1 to 20. This ball decides which decision situation becomes payoff-relevant for the respective participant. Every decision situation is drawn with the same probability. The experimenter will type in this number at the assistant's computer.

Finally the assistant draws one ball out of each of four nontransparent bags. Every bag corresponds to one of the four types of urns.

- Bag 1 contains 5 white balls and 5 black balls; corresponds to an urn of type 1
- Bag 2 contains 8 white balls and 2 black balls; corresponds to an urn of type 2
- Bag 3 contains 2 white balls and 8 black balls; corresponds to an urn of type 3
- Bag 4 contains 2 white balls and 6 black balls; corresponds to an urn of type 4

The draw from bag 1 (2,3,4) decides which color will be paid out for an urn of the type 1 (2,3,4). At the assistant's computer the experimenter types in which color has been drawn from the four bags.

For example: if, in the third draw, the assistant draws a ball with the number 2, the decision situation 2 becomes payoff-relevant for participant 3. If, in decision situation 2, there is only one urn which is of type 1, the color of the ball which has been drawn from bag one pins down the payoff of participant 3.

Assume this decision situation is exactly the decision situation depicted above, which is of type 1. If the assistant has drawn a white ball from bag 1, participant 3 earns 20 Euro if he chose option A in this decision situation; he earns 15 Euro if he chose option B. If the assistant has drawn a black ball from bag 1, the participant earns 0 Euro if he chose option A and 5 Euro if he chose option B.

Please note: as every decision situation will be drawn with the same probability, it is in your interest to take every decision carefully.

Subsequently the computer computes your income, which will be shown to you on your screen. Furthermore you will be informed, which part and which decision situation have been drawn for you as well as which color decides your income.

Part II

Groups

At the beginning of part II you will be randomly matched with another participant of this experiment. The two of you will form one group in part II. Groups will remain unchanged for the rest of part II.

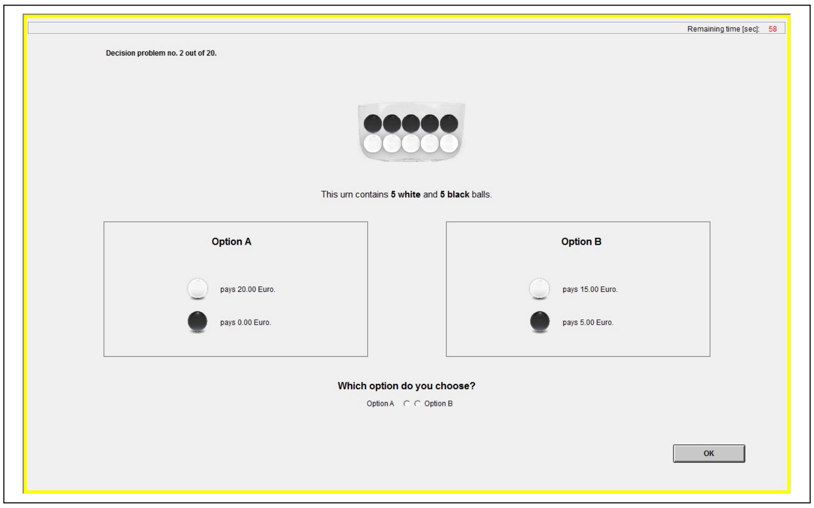
Every participant will be randomly assigned by the computer one of two roles in his group. We call these roles person 1 and person 2. At the beginning you will be informed on your screen whether you will be person 1 or person 2 for the rest of part II.

Task

In this part person 1 and person 2 will be presented 20 decision situations. These decision situations will be identical to the decision situations from part I. The sequence of decision situation however, will be different from part I. As in part I, both as person 1 and person 2, you will be informed on your screen about the value of a black ball and the value of a white ball in the case you choose option A and option B.

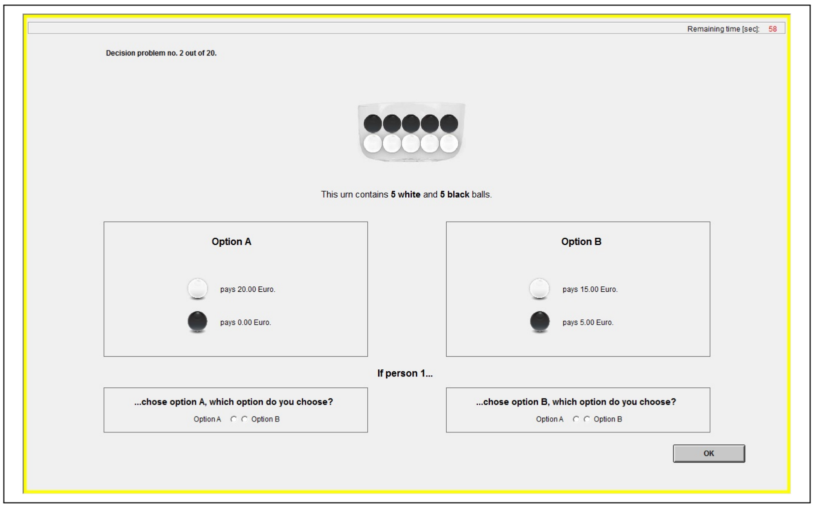
In every decision situation each participant chooses one of the two options. Person 1 will take the decisions as in part I. Person 2 can make his decisions conditionally on the choice of person 1. To do this, person 2 is asked to take a decision for the case that person 1 chose option A and for the case that person 1 chose option B. Person 1 and person 2 decide simultaneously and only at the end of the experiment person 2 will be informed about the choice of person 1 in this decision situation.

This is how the screen of person 1 might look like:



Example - Decision Problem - Person 1

This is how the screen of person 2 might look like:



Example - Decision Problem - Person 2

You take your decision by marking either option A or option B on the screen. Your decision is final once you clicked the OK-button in the lower part of the screen.
Please consider your decision carefully, as your choice can – as described below – affect your payoff.

Payoff
After all participants completed their decision problems the assistant will be selected randomly by the computer. As described in the instructions of part I, for deciding whether part I or part II becomes payoff relevant, the assistant draws one ball from a nontransparent bag containing two balls.
Assume that part II is drawn as being payoff-relevant. Then, for each group, the assistant draws one ball out of a nontransparent bag which contains 20 balls numbered from 1 to 20. This ball decides which decision situation becomes payoff-relevant for the participants of the respective group. Every decision situation is drawn with the same probability. The experimenter will type in this number at the assistant's computer.
Finally the assistant draws one ball out of each of four nontransparent bags. Every bag corresponds to one of the four types of urns.

- Bag 1 contains 5 white balls and 5 black balls; corresponds to an urn of type 1
- Bag 2 contains 8 white balls and 2 black balls; corresponds to an urn of type 2
- Bag 3 contains 2 white balls and 8 black balls; corresponds to an urn of type 3
- Bag 4 contains 2 white balls and 6 black balls; corresponds to an urn of type 4

The draw from bag 1 (2,3,4) decides which color will be paid out for an urn of the type 1 (2,3,4). At the assistant's computer the experimenter types in which color has been drawn from the four bags.
Assume this decision situation is exactly the decision situation depicted above, which is of type 1. If the assistant has drawn a white ball from bag 1, person 1 and person 2 of group 5 receive

the following income: If person 1 and person 2 both chose option A each receives 20 Euro. If both chose option B, each receives 15 Euro. If person 1 chose option A and person 2 chose option B, person 1 receives 20 Euro and Person 2 15 Euro. Analogously if person 1 chose option B and person 2 chose option A, person 1 receives 15 Euro and person 2 receives 20 Euro.
If the assistant has drawn a black ball from bag 1, person 1 and person 2 of group 5 receive the following income: If person 1 and person 2 both chose option A, each receives 0 Euro. If both chose option B, each receives 15 Euro. If person 1 chose option A and person 2 chose option B, person 1 receives 0 Euro and Person 2 15 Euro. Analogously if person 1 chose option B and person 2 chose option A, person 1 receives 15 Euro and person 2 receives 0 Euro.
Subsequently the computer computes your income. You will be informed on your screen, which part and which decision situation have been drawn for you as well as which color defines your income. As person 1 you will be shown both options, your choice, the resulting income as well as the final choice of person 2 and the resulting income of person 2. As person 2 you will also be shown both options, the choice of person 1 and which final choice results for yourself. This defines your resulting income.
You will then be informed about the amount of Euro you have earned in this experiment. You will also be informed about how much the other group member earned in the experiment.

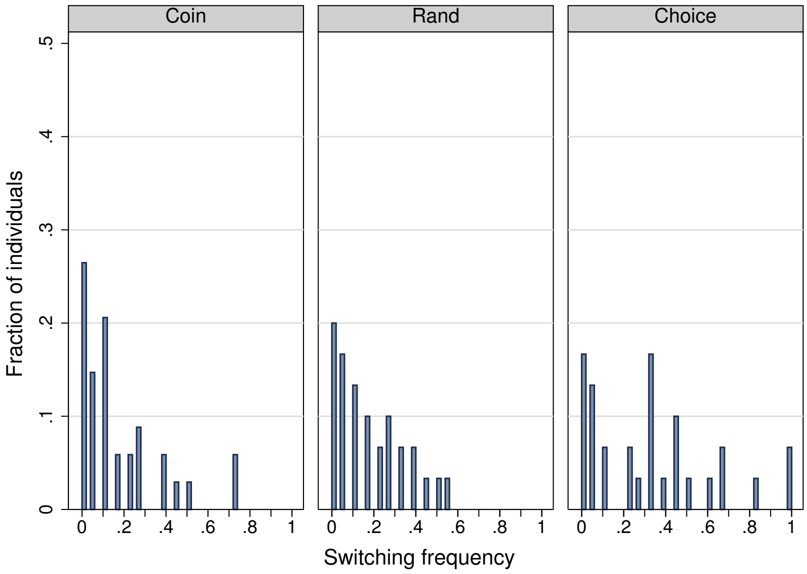
A.3 Additional results

A.3.1 Supplementary tables and figures

A: 20,0 vs. B:	Coin	Rand	Choice	χ^2 p-value
Panel A: 20/80 Lotteries				
(0.8,1)	86.8%	95.0%	86.7%	0.038
(5.6,0.2;0.6,0.8)	72.1%	73.3%	68.3%	0.936
(4,1)	27.9%	41.7%	28.3%	0.316
(8,0.2;3,0.8)	17.6%	43.3%	18.3%	0.004
(7.2,1)	7.4%	3.3%	3.3%	0.595
(10.4,0.2;5.4,0.8)	4.4%	5.0%	6.7%	0.815
Panel B: 50/50 Lotteries				
A: 20,0 vs. B:	Coin	Rand	Choice	χ^2 p-value
(8,1)	17.6%	20.0%	28.3%	0.146
(11,0.5;6,0.5)	19.1%	16.7%	23.3%	0.704
(10,1)	2.9%	13.3%	3.3%	0.066
(12.5,0.5;7.5,0.5)	5.9%	10.0%	8.3%	0.857
(12,1)	0.0%	3.3%	6.7%	0.212
(14,0.5;9,0.5)	1.5%	1.7%	5.0%	0.294
Panel C: 80/20 Lotteries				
A: 20,0 vs. B:	Coin	Rand	Choice	χ^2 p-value
(15.2,1)	19.1%	13.3%	18.3%	0.791
(16.4,0.8;11.4,0.2)	19.1%	11.7%	15.0%	0.61
(16,1)	11.8%	8.3%	11.7%	0.897
(17,0.8;12,0.2)	20.6%	11.7%	16.7%	0.512
(16.8,1)	8.8%	1.7%	11.7%	0.096
(17.6,0.8;12.6,0.2)	7.4%	5.0%	11.7%	0.577

Table A.1: Frequency of lottery A choices of first and second mover in Part I

Note: χ^2 test is used to test for differences between choices in treatments Coin, Rand and Choice.



Note: Switching takes value 1 if the second mover changes his choice in Part II for at least one of the possible choices of the first mover with respect to the choice made in Part I for the same decision.

Figure A.1: Distribution of individual switching rates, by treatment

			$p = 0.2$			$p = 0.5$			$p = 0.8$		
c level	f level		**Coin**	**Rand**	**Choice**	**Coin**	**Rand**	**Choice**	**Coin**	**Rand**	**Choice**
c=20	1.2	Imitate	2.9%	10.0%	16.7%	2.9%	20.0%	16.7%	0.0%	3.3%	20.0%
		Deviate	2.9%	0.0%	0.0%	2.9%	0.0%	0.0%	8.8%	3.3%	3.3%
		Revise	5.9%	10.0%	26.7%	17.6%	6.7%	16.7%	14.7%	20.0%	6.7%
		No change	88.2%	80.0%	56.7%	76.5%	73.3%	66.7%	76.5%	73.3%	70.0%
c=15	1.2	Imitate	2.9%	0.0%	23.3%	11.8%	13.3%	13.3%	5.9%	10.0%	26.7%
		Deviate	0.0%	0.0%	6.7%	5.9%	3.3%	0.0%	0.0%	3.3%	0.0%
		Revise	17.6%	20.0%	23.3%	20.6%	3.3%	10.0%	14.7%	10.0%	13.3%
		No change	79.4%	80.0%	46.7%	61.8%	80.0%	76.7%	79.4%	76.7%	60.0%
c=20	1	Imitate	11.8%	13.3%	20.0%	0.0%	10.0%	26.7%	2.9%	16.7%	13.3%
		Deviate	8.8%	0.0%	3.3%	2.9%	3.3%	0.0%	2.9%	0.0%	0.0%
		Revise	14.7%	23.3%	16.7%	5.9%	6.7%	3.3%	8.8%	3.3%	20.0%
		No change	64.7%	63.3%	60.0%	91.2%	80.0%	70.0%	85.3%	80.0%	66.7%
c=15	1	Imitate	2.9%	13.3%	23.3%	0.0%	6.7%	16.7%	5.9%	13.3%	13.3%
		Deviate	5.9%	0.0%	0.0%	5.9%	0.0%	0.0%	0.0%	0.0%	3.3%
		Revise	17.6%	16.7%	13.3%	5.9%	0.0%	6.7%	11.8%	10.0%	13.3%
		No change	73.5%	70.0%	63.3%	88.2%	93.3%	76.7%	82.4%	76.7%	70.0%
c=20	0.8	Imitate	2.9%	10.0%	23.3%	0.0%	3.3%	16.7%	2.9%	3.3%	23.3%
		Deviate	2.9%	0.0%	0.0%	0.0%	3.3%	0.0%	0.0%	0.0%	0.0%
		Revise	0.0%	3.3%	3.3%	0.0%	0.0%	6.7%	14.7%	3.3%	13.3%
		No change	94.1%	86.7%	73.3%	100.0%	93.3%	76.7%	82.4%	93.3%	63.3%
c=15	0.8	Imitate	0.0%	3.3%	20.0%	2.9%	3.3%	16.7%	5.9%	6.7%	23.3%
		Deviate	2.9%	3.3%	0.0%	5.9%	0.0%	0.0%	0.0%	0.0%	0.0%
		Revise	8.8%	3.3%	6.7%	0.0%	0.0%	0.0%	5.9%	6.7%	20.0%
		No change	88.2%	90.0%	73.3%	91.2%	96.7%	83.3%	88.2%	86.7%	56.7%

Table A.2: Strategy choices in Part II

A.3.2 Estimating social utility: Econometric specification & results

We structurally estimate two distinct models of social utility and test their relative fit, based on decisions in Rand and Choice. In both models we assume a CRRA (consumption) utility function with parameter r, i.e. $u(x) = x^r$. We estimate r for each subject individually based on his choices in Part I.[1] Under relative payoff concerns, we assume social utility to be given by a comparison term, in which social losses enter negatively and are weighted by the parameter $\lambda \geq 0$; see Appendix A.1 for details. If social losses loom larger than social gains, then $\lambda > 1$. In the estimation we test for differences in λ between Rand and Choice, for choice-dependency of relative payoff concerns. In a model based on social comparison theory we assume social utility to be given by a constant parameter γ (see Appendix A.2 for details). We also allow for differences in γ between Rand and Choice.

Econometric specification

Following Hey and Orme (1994), we allow subjects to make so-called Fechner errors when comparing expected utilities (also see, e.g. von Gaudecker et al., 2011; Loomes, 2005). Hence, a subject chooses lottery i if and only if $V_{i,k} - V_{-i,k} > \tau\epsilon$, where $V_{i,k}$ is the expected overall utility if his peer has lottery k; ϵ is drawn from a standard logistic distribution and assumed to be independent between subjects and decisions. The parameter $\tau > 0$ serves as a scaling factor and is assumed to be constant across subjects and decisions. The probability to choose lottery i in decision problem t ($t = 1, \ldots, 18$) depends on the parameters $\theta = (r, \lambda, \gamma)$ and τ. The likelihood for subject n ($n = 1, \ldots, N$) can be determined by the score function $d_{n,t}(i|\theta, \tau) = \frac{1}{\tau}(V^t_{i,k}(\theta) - V^t_{-i,k}(\theta))$. Then, the likelihood function becomes $\mathcal{L}_{n,t}(i|\theta, \tau) = \Lambda\left(d_{n,t}(i|\theta, \tau)\right)$, where $\Lambda(x) = (1 + \exp(-x))^{-1}$ denotes the standard logistic cumulative distribution function. The log-likelihood function to be maximized is simply $L(\theta, \tau) = \sum_{n,t} \ln \mathcal{L}_{n,t}(c_t|\theta, \tau)$, where we sum over all subjects n, and their choices c_t in all decision problems t. It is maximized using the Broyden-Fletcher-Goldfarb-Shanno (BFGS) algorithm (see, e.g., Broyden, 1970; Fletcher, 1970). Table A.3 reports the estimation results.[2] Columns (1) and (2) describe the estimated models of relative payoff concurs, columns (3) and (4) estimation results for models of social comparison theory.

In models (1) and (2) λ is significantly larger than one (p-value = 0.008 and 0.022,

[1] Results remain qualitatively the same if we only use data from Part II and estimate an average r.

[2] By pooling all observations from second movers in treatments Rand and Choice irrespectively of whether they are actually affected by the presence of their peer, this approach yields the most conservative estimates of the social utility parameters. We also ran estimations, in which we dropped observations for second movers who never switched in any decision problem (six subjects in Rand and five subjects in Choice, corresponding to 20.0% and 16.7% of all choices, respectively). Estimates of λ and γ only increase slightly, but significance levels stay exactly the same.

	(1)	(2)	(3)	(4)
λ	$1.1785^{\dagger\dagger\dagger}$	$1.1135^{\dagger\dagger}$		
	[0.0674]	[0.0494]		
λ_C		1.1163		
		[0.0966]		
γ			0.2567***	0.1535***
			[0.0696]	[0.0503]
γ_C				0.2019*
				[0.1173]
Error parameter				
τ	2.6065***	2.6021***	0.6293***	0.6265***
	[0.2719]	[0.2689]	[0.0863]	[0.0857]
Observations	2160	2160	2160	2160
Pseudo log–lik.	-1280.95	-1279.33	-1049.69	-1045.21

Note: In models (1)-(4) r is fixed to individual estimates for each subjects' Part I choices, where $r > 0$ is assumed throughout. Models (1) and (2) report estimates of λ, assuming $\lambda > 0$. In (2) we include a treatment dummy for Choice, λ_C, which enters multiplicatively into λ. In (3) and (4) we estimate a conformism parameter γ. In model (4), the treatment dummy γ_C enters additively.
The scaling parameter τ refers to the Fechner error. Standard errors are reported in brackets and clustered on a subject level; * (**, ***) indicates significant difference from 0, † (††, † † †) indicates significant difference from 1, at the 1% (5%, 10%) level, respectively.

Table A.3: Structural estimation of social utility models

respectively), suggesting that subjects are generally loss averse with respect to their peer's outcome. In model (2) we control for treatment differences by introducing a dummy λ_C for Choice, which enters multiplicatively into λ.[3] λ_C is not significantly different from one (p-value = 0.229), suggesting that relative payoff concerns remain unchanged between Rand and Choice.

In models (3) and (4) we find that γ is significantly larger than zero in both treatments, consistent with imitation being the most frequent strategy in Rand and Choice (conditional on switching). Moreover, column (4) shows that γ is significantly larger in Choice than in Rand, in line with the assumption that peers' choices provide a stronger anchor than random allocations.[4]

Which model fits our data best? Comparing the log-likelihoods of model (1) and (2) versus (3) and (4) suggests that the latter models might provide a better fit. This is confirmed using the Vuong test (Vuong, 1989). In terms of goodness of fit, we find that model (3) significantly outperforms model (1) as does model (4) in comparison to model (2) (both p-values<0.01).

[3]This is due to our constraint $\lambda > 0$ which is implemented by estimating $\lambda = \exp(\ln \lambda)$.

[4]This also holds if γ and γ_C are estimated while controlling for relative payoff concerns, fixing $\hat{\lambda}$ estimated in model (1).

Appendix B

Conflicting Risk Attitudes

B.1 A stylized model of bargaining over risk

We present a stylized model of bargaining over risk to illustrate how heterogeneity in risk attitudes could be related to conflict. Consider two individuals i and j who bargain over the level of investment into a risky asset, e.g. new machinery for farming. The investment is indivisible and has to be agreed upon by the two parties. If no agreement is reached, conflict arises, and both individuals are left with the outside option d. If both individuals agree on investing x where $x \in [1, 6]$, the investment yields $6 + x$ with probability p and $6 - x$ with probability $1 - p$, where $p \geq 0.5$, for each individual. These assumptions are based on the payoffs of the lotteries used in the experiment.

Each individual's utility function is CRRA, with $u = z^{1-\gamma}$, where γ is private information.[1] For simplicity, we assume there are only two types of individuals, those with high risk aversion and those with low risk aversion, γ_H with probability q and γ_L with probability $1 - q$. We will further consider only two levels of investment, high and low, i.e. x_H and x_L. For the low risk averse individuals, $u(x_H(\gamma_L)) > u(x_L(\gamma_L)) > u(d)$. For the high risk averse individuals, $u(x_L(\gamma_H)) > u(d) > u(x_H(\gamma_H))$. Hence, while low risk averse individuals prefer a high investment, but also accept a low investment, high risk averse individuals prefer a low investment, and do not accept the high investment, as they then favor the outside option.

In the bargaining game, one player is randomly assigned to be the proposer, who makes a take-it-or-leave-it offer to the responder. If the proposal x is accepted, the investment is made and both parties share the proceeds equally. If the proposal is rejected, the relationship between the two parties becomes conflictive. If a high risk averse individual is the proposer, the prediction is clear. He proposes the low investment and both a high risk averse and low risk averse responder accept.

[1] If each individual were to choose the investment amount, x, individually, his optimal level of investment would be a decreasing function of γ.

Proposition 1. If the low risk averse individual is the proposer, there exists a threshold $\hat{\gamma}$ such that if $\gamma < \hat{\gamma}$, the proposer chooses x_H and the offer gets rejected with probability q. In those situations, conflict arises.

Proof. For the low risk averse individual, the expected utility from proposing x_H is $q \cdot u(x_H(\gamma_L)) + (1-q)d$. The utility from proposing x_L is $u(x_L(\gamma_L))$. For the individual to prefer a proposal of x_H, the following condition needs to hold:

$$q \cdot u(x_H(\gamma_L)) - u(x_L(\gamma_L)) > -(1-q)u(d). \tag{B.1}$$

We show that the marginal utility of investment x is decreasing in γ, so that for smaller γ the difference in utility between x_H and x_L is larger. Hence, γ needs to be sufficiently small for (2) to hold. First note that, the derivative of $u(x(\gamma))$ with respect to x is:

$$\frac{\partial u(x(\gamma))}{\partial x} = (1-\gamma)[p(6+x)^{-\gamma} - (1-p)(6-x)^{-\gamma}],$$

which is positive, since $p \geq 0.5$. Now we turn to the second derivative with respect to γ,

$$\begin{aligned}\frac{\partial^2 u(x(\gamma))}{\partial x \partial \gamma} = & - \quad [p(6+x)^{-\gamma} - (1-p)(6-x)^{-\gamma}] \\ & - \quad (1-\gamma)[p(6+x)^{-\gamma}(ln(6+x)) - (1-p)(6-x)^{-\gamma}(ln(6-x))],\end{aligned}$$

which is negative.

B.2 Sironko district and the Bagisu people

This study was conducted in the Sironko district, located in the eastern part of the Eastern region of Uganda. Sironko district is a densely populated area with an estimated population of 346,400, roughly 284 inhabitants per squared kilometer, around 90% of whom live in rural areas (Ministry of Water & Environment Uganda, 2010). People primarily earn their livelihoods with farming, and soil conditions mostly favor food crops such as beans, groundnuts, maize, soya or potatoes. Most people in Sironko district belong to the ethnical group of the Bagisu people.[2] This group was discovered by ethnologists to be very vulnerable in two particular respects. Bagisu are known to suffer from violent conflicts and anger, primarily among males within kinship groups (Roscoe, 1924; La Fontaine, 1959; Heald, 1998). Further, this is predominantly caused by a particular gender ideal of male providers. Men are expected to have absolute control in marital relationships and support their kin, as well as to be resistant against violent pain, for example, in

[2]In our sample, more than 95% belong the Bagisu, who might also alternatively be referred to as Gisu people, Gishu, Masaba, or Sokwia, and are closely related to the Bukusu tribe in Kenya.

circumcision practices, a salient tradition among the Bagisu. Although households seem to be controlled by men and domestic violence is a common feature of marriage (Karamagi et al., 2006), the reputation of Bagisu men strongly depends on being married as a signal for adult masculinity. While Bagisu women are relatively free to divorce from their husband and get remarried, marital failure might imply serious consequences for Bagisu men. Bachelors or divorced men are likely to be ridiculed in their social group. Outside marriage, conflicts among kin, often refer to access to resources, conflicts over land, or land boundaries. Inheritance upon the death of a family head or the distribution of remittances are other common sources behind disputes. Conflicts might also result from accusations of sorcery if people feel that they are befallen by misfortune, or result from political disputes or gossiping. Generally, different sources of conflict might even translate into accusations of witchcraft or theft (Heald, 1998), and finally lead to violent punishment.

B.3 Experimental details

B.3.1 Instructions for the experiment

Welcome. Thank you for taking the time to come today. You can ask any of us questions during today's programme.

We have invited you here, today, because we want to learn about how people in this area take decisions. You are going to be asked to take decisions about money. The money that results from your decisions will be yours to keep.
What you need to do will be explained fully in a few minutes. But first we want to make a couple of things clear.

First of all, this is not our money. We belong to a university, and this money has been given to us for research.
Participation is voluntary. You may still choose not to participate in the exercise.
We also have to make clear that this is research about your decisions. Therefore you cannot talk with others. This is very important. I'm afraid that if we find you talking with others, we will have to send you home, and you will not be able to earn any money here today. Of course, if you have questions, you can ask one of us. We also ask you to switch off your mobile phones.
Make sure that you listen carefully to us. You will be able to make a good amount of money here today, and it is important that you follow our instructions.

During today's programme, you will be asked to make one or more choices, which will be explained to you very clearly. If you are asked to take more than one decision, only one decision will be selected to determine the money you will be paid. In that case, at the end of the exercise

we will randomly select one of your decisions to be paid out. Any money you earn will be paid out to you privately and confidentially after all parts of the exercise are complete.
Now, before we explain what you need to do, it is really important to bear one more thing in mind. You will be asked to take decisions that are not a matter of getting it right or wrong; they are about what you prefer. However, it is important to think seriously about your choices because they will affect how much money you can take home.

Part I

Task
The task consists of two parts. In the first part you will have to make one choice, which may be communicated to other participants. The choice is between the different options on the table in front of you. You can choose exactly one of these options. Each option consists of one group of 5 counters, where 4 of the counters are white and 1 is green. Next to each group of counters is a piece of paper which states how much each counter is worth in that option.

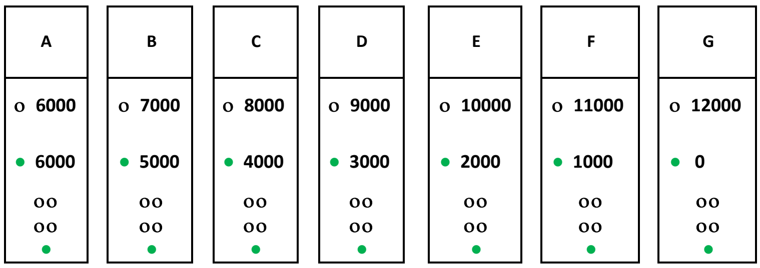


Figure B.1: Presentation of lotteries

In option A, each white counter is worth 6000 UGS and each green counter is worth 6000 UGS.
In option B, each white counter is worth 7000 UGS and each green counter is worth 5000 UGS.
In option C, each white counter is worth 8000 UGS and each green counter is worth 4000 UGS.
In option D, each white counter is worth 9000 UGS and each green counter is worth 3000 UGS.
In option E, each white counter is worth 10,000 UGS and each green counter is worth 2000 UGS.
In option F, each white counter is worth 11,000 UGS and each green counter is worth 1000 UGS.
In option G, each white counter is worth 12,000 UGS and each green counter is worth 0 UGS.

You will be asked to select one option. After you have made your choice, your earnings will be calculated in the following way. We will place the counters from the option you selected into a bag and pick one out without looking. The colour of this counter will determine the amount of money you will get. As there are 4 white counters and only 1 green counter, it is much more likely that you will pick a white counter, than a green counter.

Let me give you the following examples.

- If you chose option A and picked a white counter, how much would you go home with? (6000 UGS). If you chose option A and picked a green counter, how much would you go home with? (6000 UGS).

- If you chose option G and picked a white counter, how much would you go home with? (12,000 UGS). If you chose option G and picked a green counter, how much would you go home with? (0 UGS).
- If you chose option B and picked a green counter, how much would you go home with? (5000 UGS). If you chose option B and picked a white counter, how much would you go home with? (7000 UGS).
- If you chose option F and picked a white counter, how much would you go home with? (11,000 UGS). If you chose option F and picked a green counter, how much would you go home with? (1000 UGS).
- If you chose option C and picked a green counter, how much would you go home with? (4000 UGS). If you chose option C and picked a white counter, how much would you go home with? (8000 UGS).
- If you chose option E and picked a white counter, how much would you go home with? (10,000 UGS). If you chose option E and picked a green counter, how much would you go home with? (2000 UGS).
- If you chose option D and picked a green counter, how much would you go home with? (3000 UGS). If you chose option D and picked a white counter, how much would you go home with? (9000 UGS).

To make your decision we will use the following decision card. It shows the same 7 options as the ones presented on the table. Out of these 7 options we ask you to select one.

Control Questions

We will now ask some questions to see whether you understood the instructions.

1. If you chose option C, how much would you go home with if you picked a white counter out of the bag?
2. If you chose option A, how much would you go home with if you picked a white counter out of the bag?
3. If you chose option F, how much would you go home with if you picked a green counter out of the bag?
4. If you chose option D, how much would you go home with if you picked a white counter out of the bag?

If you have no further questions, we will now begin. Please indicate the option you choose. Remember, there are no wrong choices, so you should choose the option that you prefer.

We emphasize that it is important that you make your choice in private. Do not show your decision card to the other participants. If you need assistance, please raise your hand so that one of us can come to you to assist you. Once you have made your choice, please fold the decision card and raise your hand so that we can come and collect your decision card.

B.3.2 Details on non-reported experimental tasks

After the experimental task which we analyze in this paper, we implemented a second task designed to examine the effect of feedback about others' choices on individual choices. Based on the data collected in the social survey we hereby focus on whether different social links (such as kin, friends, or strangers) might relate to differently strong peer effects, and whether different social links might induce different strategies (such as imitation or deviation).

In the beginning of the second part, subjects were randomly paired with another participant ("peer") from the same session. One subject from each pair was then allowed to once more choose between the same options as in the first part. However, before making this choice, the subject would get to know the decision made by the peer in the first part. We implemented three different treatments. In the first treatment, within each pair, peers come from the same village, and the decision-maker gets to know the identify of the peer. In the second treatment, within each pair, peers come from the same village, but the decision-maker does not get to know the identity of the peer. In the third treatment, within each pair, peers come from different villages, and the decision-maker does not get to know the identity of his peer.

For the decision-makers in the second part, either the first or the second task is randomly selected for payment at the end of the experiment. For those subjects who acted as peers in the second part, the first task is relevant for payment. The existence of a second part and the fact that only one part would be payoff relevant was common knowledge from the very beginning of the experiment. However, subjects did not know what to expect in the second task when facing the first task (also see the instructions in section B.3.1).

B.4 Supplementary tables

Label	Question	Notes
Know	This person lives in your village. Do you know him/her?	0=No, 1=Yes
Years-rel	For how many years do you know this person?	
Close-friends	Are you close friends?	0=No, 1=Yes
Social-conflict	Are you getting along well?	0=No, 1=Yes
	(in script for interview: the purpose of this question is to find out whether there have been some issues)	
Kin	Are you related?	
	Categories: No, Yes s/he is my brother/sister, father/mother,son/daughter, grandfather/grandmother, uncle (mother's brother), auntie (father's sister), maternal uncle (mother's sister)/paternal uncle (father's brother), cousin brother/ cousin sister,nephew/niece through mother, through father, father/mother in-law brother/sister in-law,son/daughter in-law	
Neighbors	Are you neighbors?	0=No, 1=Yes
Social group	Do you belong to the same group?	
	Categories: No, same saving group, burial society,friendship group, farmer's group, microfinance group, drinking group,religious group, other	
Same religion	Do you go to the same church or mosque?	0=No, 1=Yes
Recipient	In the past 12 months, did s/he give you or anyone in your household a gift in cash or kind, or lend you or anyone in your household money or other things?	
	Categories: No, Yes gift, Yes lent money or other things,Do not know	
Recipient-purpose	What was the main purpose of the gift/loan?	
Recipient-person	Was it meant for someone specifically in the household?	
	Who was the main recipient?	
Donor	In the past 12 months, did you or anyone else in your household give her/him a gift in cash or in kind, or lend her/him money or other things?	
	Categories: No, Yes gift, Yes lent money or other things,Do not know	
Donor-purpose	What was the main purpose of the gift/loan?	
Donor-person	Was it meant for someone specifically in the household?	
	Who was the main donor?	

Table B.1: Social ties survey

	All		Analyzed sample	
N	275		252	
	Mean	Std. Dev.	Mean	Std. Dev.
Gender	0.49	0.50	0.46	0.50
Age	40.78	13.56	40.23	13.36
Household Head	0.61	0.49	0.62	0.49
Married	0.80	0.40	0.81	0.39
Number of people in household	5.96	2.77	6.04	2.77
Farming as primary occupation	0.85	0.36	0.85	0.36
Farming activities	0.96	0.20	0.96	0.20
Education (type)	1.09	0.58	1.13	0.57
Years of schooling	4.97	2.94	5.21	2.87
Education type	Freq. (%)		Freq. (%)	
None	11.3		9.1	
Primary	69.8		70.2	
Secondary	17.5		19.1	
Tertiary	1.5		1.6	
Religion	Freq. (%)		Freq. (%)	
Catholicism	38.2		38.9	
Protestantism (Anglicanism & other)	39.7		39.3	
Islam	11.3		11.5	
Seventh Day Adventists	0.4		0.4	
Born Again	10.6		9.9	

Table B.2: Summary statistics for *whole* and *analyzed* sample

Note: The whole and analyzed sample only differ in the observations which were dropped due to failures in the control questions in the beginning of the experiment.

KIN				
N	KIN	NONKIN	Other	Total
All	351	566	1	918
No conflict	293	427	.	720
Conflict	58	139	.	197
Freq. (%)				
All	38.24%	61.66%	0.11%	100%
No conflict	83.48%	75.44%	.	78.43%
Conflict	16.52%	24.56%	.	21.46%

GENERATION – among kin				
N	Same Generation	Junior/Senior		Total
All	195	156		351
No conflict	166	127		293
Conflict	29	29		58
Freq. (%)				
All	55.56 %	44.44 %		100 %
No conflict	85.13 %	81.41 %		83.48 %
Conflict	14.87 %	18.59 %		16.52%

GENDER				
N	Female-Female	Male-Male	Female-Male	Total
All	195	253	470	918
No conflict	155	221	344	720
Conflict	39	32	126	197
Do not know each other	1	0	0	1
Freq. (%)				
All	21.24 %	27.56 %	51.20 %	100 %
No conflict	79.49 %	87.35 %	73.19 %	78.43 %
Conflict	20.00%	12.65 %	26.81 %	21.46 %
Do not know each other	0.51 %	0.00 %	0.00 %	0.11 %

Table B.3: Dyadic dataset

Note: Numbers report the number of *dyads* in our sample. A *dyad* defines a pair of two individuals, for whom we elicited their social tie in the social survey.
In total, the analyzed sample consists of 918 dyads. From the whole sample, which includes 1,096 dyads, we drop all dyads in which at least one individual failed to pass the control task. In one particular dyad, both persons agreed that they do not know each other, hence, they do not appear in the categories w.r.t. kin.

Variable	Mean	SD	N
Difference in RA (δ^{RA})	2.23	1.63	917
Distance in age	15.52	11.03	917
Difference in gender	0.51	0.50	917
Different tribes	0.10	0.30	917
Different marital status	0.28	0.45	917
Different religion	0.51	0.50	917
Distance in education	0.52	0.61	917
Difference in wealth index	1.93	1.79	917
Different occupation	0.30	0.46	917
Difference in disabilities	0.42	0.49	917
Neighbors	0.60	0.49	917
Different social groups	0.57	0.50	917
Loan	0.19	0.39	917
Gifts	0.45	0.50	917
Kin	0.38	0.49	917

Table B.4: Independent variables in dyadic regression models

Note: The listed variables refer to the independent variables included in the dyadic regression models reported in sections 2.6.1 and 2.6.2.

	All	All	Not kin	Kin	Female-female	Female-male	Male-male
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Intermediate δ^{RA}	0.006	0.005	0.034	0.033	-0.08	0.065	0.107
	[0.049]	[0.049]	[0.067]	[0.059]	[0.162]	[0.086]	[0.061]
Large δ^{RA}	0.056	0.056	0.015	0.155*	0.058	0.048	0.165***
	[0.043]	[0.043]	[0.057]	[0.078]	[0.083]	[0.080]	[0.056]
Age distance	0.004***	0.004***	0.006***	0.000	0.010***	0.002	0.007**
	[0.001]	[0.001]	[0.001]	[0.002]	[0.003]	[0.002]	[0.003]
Diff. Gender	0.121***	0.114***	0.107**	0.199***			
	[0.035]	[0.034]	[0.051]	[0.045]			
Diff. Tribe	0.09	0.052	0.073	0.158	0.1	0.035	0.027
	[0.160]	[0.158]	[0.135]	[0.257]	[0.131]	[0.215]	[0.320]
Diff. Marital Status	0.028	0.034	0.064	0.062*	0.107	0.054	0.131*
	[0.043]	[0.042]	[0.057]	[0.038]	[0.076]	[0.066]	[0.071]
Diff. religion	-0.084***	-0.094***	-0.108***	-0.054	-0.02	-0.114**	-0.125
	[0.023]	[0.024]	[0.040]	[0.069]	[0.069]	[0.047]	[0.088]
Education distance	0.017	0.019	0.007	0.100*	-0.012	0.080*	0.026
	[0.024]	[0.023]	[0.034]	[0.053]	[0.079]	[0.040]	[0.068]
Wealth distance	-0.012	-0.011	0.005	-0.051**	0.076***	-0.031*	-0.006
	[0.010]	[0.011]	[0.014]	[0.020]	[0.018]	[0.016]	[0.021]
Diff. Occupation	0.04	0.045	0.049	0.089	0.100*	0.009	0.095*
	[0.050]	[0.049]	[0.066]	[0.070]	[0.056]	[0.084]	[0.055]
Diff. In Disabilities	0.027	0.033	0.053	0.068	0.085	0.056	0.016
	[0.034]	[0.032]	[0.037]	[0.055]	[0.073]	[0.045]	[0.102]
Kin		-0.091**					
		[0.043]					
Observations	715	715	447	211	115	350	164
Log-lik.	-318.6	-314.8	-214.2	-83.93	-52.93	-175.8	-58.17

Table B.5: Dyadic logit regression – dummies for small and large differences

Note: This table reports coefficients estimates from a dyadic logit regression on the presence of a conflict link. Intermediate δ^{RA} is a dummy variable that takes value 1 if $1 < \delta^{RA} \leq 3$, zero otherwise; large δ^{RA} is a dummy with value 1 if $\delta^{RA} \geq 4$. The omitted category, small δ^{RA}, takes value 1 if $\delta^{RA} < 2$. Other independent variables are defined as in Table 2.6. Dyad characteristics are dropped due to convergence problems in the case of female-female dyads. Both regressions includes village fixed effects for both individuals per dyad. Standard errors are clustered on the village level and reported in brackets; *** p<0.01, ** p<0.05, * p<0.1.

Likelihood of conflict	All (1)	All (2)	Not kin (3)	Kin (4)	Female-female (5)	Female-male (6)	Male-male (7)
δ^{RA}	0.019*	0.019*	0.009	0.039**	-0.006	0.022	0.035**
	[0.010]	[0.010]	[0.013]	[0.019]	[0.030]	[0.018]	[0.016]
Age distance	0.003**	0.003***	0.004**	0.002	0.008**	0.002	0.009***
	[0.001]	[0.001]	[0.002]	[0.002]	[0.003]	[0.002]	[0.003]
Diff. Gender	0.120***	0.113***	0.099*	0.200***			
	[0.036]	[0.037]	[0.051]	[0.047]			
Diff. Tribe	0.111	0.076	0.177	0.175	-0.054	0.154	0.043
	[0.138]	[0.139]	[0.126]	[0.156]	[0.220]	[0.198]	[0.275]
Diff. Marital Status	0.045	0.051	0.064	0.006	0.117	0.027	0.051
	[0.037]	[0.036]	[0.048]	[0.041]	[0.088]	[0.053]	[0.102]
Diff. Religion	-0.067***	-0.076***	-0.079**	-0.027	0.085	-0.115**	-0.11
	[0.024]	[0.024]	[0.036]	[0.054]	[0.059]	[0.047]	[0.072]
Education distance	0.021	0.022	0.015	0.038	0.069	0.061	-0.003
	[0.022]	[0.022]	[0.027]	[0.050]	[0.064]	[0.038]	[0.049]
Wealth distance	-0.004	-0.002	0.001	-0.004	0.063***	-0.016	-0.009
	[0.009]	[0.010]	[0.013]	[0.027]	[0.022]	[0.018]	[0.020]
Diff. Occupation	0.033	0.04	0.033	0.042	0.051	0.013	0.05
	[0.044]	[0.044]	[0.064]	[0.070]	[0.063]	[0.067]	[0.054]
Diff. In Disabilities	0.052	0.059*	0.053	0.078*	0.007	0.062*	0.027
	[0.033]	[0.031]	[0.036]	[0.042]	[0.103]	[0.033]	[0.106]
Kin		-0.096**					
		[0.045]					
% conflict	21.46%	21.46%	24.56%	16.52%	20.00%	26.81%	12.65%
Observations	839	839	521	251	133	403	188
Log-lik.	-372.9	-367.9	-242.4	-81.94	-53.84	-187.3	-54.47

Table B.6: Dyadic logit regression – without social link characteristics

Note: This table reports marginal effects from a dyadic logit regression on the presence of a conflict link. Independent variables are defined as in Table 2.6. Both regressions includes village fixed effects and session fixed effects for both individuals per dyad. Standard errors are clustered on the village level and reported in brackets; *** p<0.01, ** p<0.05, * p<0.1.

Likelihood of conflict	All (1)	All (2)	Not kin (3)	Kin (4)	Female-female (5)	Female-male (6)	Male-male (7)
δ^{RA}	0.132*	0.154*	0.0278	0.502**	0.00924	0.173	0.521**
	[0.0732]	[0.0899]	[0.104]	[0.213]	[0.248]	[0.142]	[0.223]
Age distance	0.0213**	0.0253***	0.0260***	0.0303	0.0883***	0.0111	0.117***
	[0.00844]	[0.00789]	[0.00983]	[0.0188]	[0.0285]	[0.0119]	[0.0439]
Diff. Gender	0.837***	0.975***	0.776**	2.512***			
	[0.265]	[0.252]	[0.325]	[0.545]			
Diff. Tribe	0.773	0.457	0.902	2.476	-1.602	1.100	2.409
	[0.969]	[0.993]	[1.046]	[1.784]	[2.392]	[1.326]	[3.467]
Diff. Marital Status	0.314	0.277	0.151	-0.237	1.703**	-0.0794	1.226
	[0.257]	[0.308]	[0.382]	[0.511]	[0.765]	[0.456]	[0.862]
Diff. Religion	-0.463***	-0.679***	-0.815***	-0.143	0.291	-1.082***	-1.027
	[0.180]	[0.185]	[0.268]	[0.551]	[0.605]	[0.375]	[1.234]
Education distance	0.144	0.0518	-0.147	0.102	1.019	0.136	0.267
	[0.156]	[0.192]	[0.214]	[0.516]	[0.755]	[0.277]	[0.984]
Wealth distance	-0.0263	-0.0247	0.0150	-0.0508	0.547***	-0.0955	-0.323*
	[0.0657]	[0.0612]	[0.0745]	[0.227]	[0.200]	[0.115]	[0.194]
Diff. Occupation	0.228	0.282	0.298	0.450	0.353	0.258	0.346
	[0.306]	[0.312]	[0.443]	[0.589]	[0.808]	[0.463]	[0.605]
Diff. In Disabilities	0.358	0.521**	0.477*	0.935	0.227	0.401	0.449
	[0.229]	[0.249]	[0.249]	[0.593]	[0.786]	[0.248]	[1.476]
Neighbors		-1.162***	-1.502***	-0.655	-0.863	-1.058***	-2.749***
		[0.252]	[0.339]	[0.557]	[0.697]	[0.381]	[0.980]
Diff. Groups		-1.364***	-1.465***	-1.624***	-0.857	-1.566***	-2.044***
		[0.319]	[0.391]	[0.436]	[0.709]	[0.493]	[0.656]
Loan		-0.242	-0.606	0.0805	-1.793*	-0.290	0.412
		[0.304]	[0.476]	[0.358]	[0.976]	[0.525]	[0.762]
Gift		-0.431	-0.476	0.794*	-1.847*	-0.612	0.146
		[0.283]	[0.383]	[0.465]	[1.005]	[0.416]	[0.826]
Kin		-0.646**					
		[0.296]					
% conflict	21.46%	21.46%	24.56%	16.52%	20.00%	26.81%	12.65%
Observations	839	839	521	251	133	403	188
Log-lik.	-372.9	-327.2	-205.7	-74.96	-46.33	-164.3	-43.83

Table B.7: Dyadic logit regression – coefficient estimates

Note: This table reports coefficients estimates from a dyadic logit regression on the presence of a conflict link. Independent variables are defined as in Table 2.6. Both regressions includes village fixed effects and session fixed effects for both individuals per dyad. Standard errors are clustered on the village level and reported in brackets; *** p<0.01, ** p<0.05, * p<0.1.

	(1)	(2)	(3)
RA	0.007		
	[0.047]		
$\frac{RA_i - RA_j}{2}$		-0.016	-0.013
		[0.041]	[0.040]
Age distance	0.009*	0.010**	0.009*
	[0.004]	[.]	[0.004]
Diff. Gender	0.083	0.245**	0.236**
	[0.185]	[.]	[0.086]
Diff. Tribe	-0.253	-0.076	-0.101
	[0.321]	[0.318]	[0.260]
Diff. Marital Status	0.06	0.063	0.083
	[0.126]	[0.050]	[0.094]
Diff. Religion	0.103	-0.079	-0.078
	[0.130]	[.]	[0.063]
Education distance	0.063	-0.057	-0.042
	[0.159]	[0.055]	[0.086]
Wealth distance	0.031	-0.022	-0.015
	[0.036]	[0.016]	[0.027]
Diff. Occupation	-0.157	0.079	0.16
	[0.329]	[0.113]	[0.141]
Diff. In Disabilities	-0.199	-0.079	-0.028
	[0.162]	[0.077]	[0.095]
Sample	$RA_i = RA_j$	$\delta_{ij}^{RA} = 1$	$\delta_{ij}^{RA} = 1$
Fixed Effects	Village + Session	Village + Session	Village
% conflict	17.14%	20.19%	20.19%
Observations	65	147	147
Log-lik.	-25.56 -59.23	-68.30	

Table B.8: Dyadic logit regression: conflicts and risk attitudes

This table reports the marginal effects of a dyadic logit regression. The dependent variable refers to the presence of conflict. Model (1) includes only those dyads for which both individuals exhibit exactly the same risk attitude ($RA_i = RA_j$); models (2) and (3) include only those dyads for which individuals differed by exactly one choice, i.e. $\delta_{ij}^{RA} = 1$. Independent variables are defined as in Table 2.6. Due to limited degrees of freedom we restrict the set of independent variables to the socio-economic characteristics.
Regressions (1) and (2) include village and session fixed effects, for both individuals in each dyad. Given limited degrees of freedom and to test the robustness of model (2), we estimate the same model, but only with village fixed effects, reported in (3).
Standard errors are clustered on the village level and reported in brackets; *** p<0.01, ** p<0.05, * p<0.1. .

B.5 Measuring differences in risk attitudes using certainty equivalents

In the experiment, subjects choose one out of seven lotteries. As given in Table 2.1 in section 2.3.2 of the main text, choosing a particular lottery corresponds to a CRRA parameter r, if one assumes an expected utility framework and constant relative risk aversion. One suggestion would be to measure differences in risk attitudes as the absolute distance in certainty equivalents, instead of the absolute distance in lottery choices.

Assume that each subject's CRRA parameter is given by a range of values $[r_1, r_2)$. For example, choosing lottery B corresponds to $r \in [2.69, 8.13)$, choosing lottery D corresponds to $r \in [1.03, 1.55)$, and choosing lottery F corresponds to $r \in [0.38, 0.70)$. For a given r, the expected utility of lottery L is given by $EU[L|r] = p \cdot \frac{x_h^{1-r}}{1-r} + (1-p) \cdot \frac{x_l^{1-r}}{1-r}$, assuming the standard functional utility under constant relative risk aversion, where x_h, x_l denote the high and low outcomes, respectively. A subject's certainty equivalent $ce(L|r_1, r_2)$ can be approximated by the midpoint between $EU[L|r_1]$ and $EU[L|r_2]$. If lottery A or G is chosen, the certainty equivalent can be approximated by the expected utility determined by the boundary values for r, as given in Table 2.1. The difference in risk attitudes for subjects i and j may then be defined by the absolute difference between ce_i and ce_j.

Note, that ce_i and ce_j are a function of some r_1 and r_2, which need to be specified. The intuitively most appealing approach would be to define the difference in risk attitudes *as perceived by subject* i, i.e., as the difference in certainty equivalents of his and j's lottery, conditional on his own CRRA parameter range: $\delta_{ij}^{CE} = |ce(L_i|r_{i,1}, r_{i,2}) - ce(L_j|r_{i,1}, r_{i,2})|$. Since not $\delta_{ij}^{CE} \neq \delta_{ji}^{CE}$, unless $L_i = L_j$, this definition of differences in risk attitudes is not symmetric. Analyzing the relation between the likelihood for conflict and differences in risk attitudes, hence, requires to examine unilateral links. As argued in the main text, defining conflicts unilaterally is not as conservative as defining conflicts by using the "or-matching" in a dyadic dataset. Especially individuals, who actually caused a conflict with somebody else, may feel reluctant to state this conflict (compare 39ff in section 2.3.1).

Appendix C

An Anatomy of Ambiguity Attitudes

C.1 A review of related preference measurements

Following the seminal Ellsberg experiments (Ellsberg, 1961) numerous studies replicated the phenomenon of ambiguity aversion over ambiguous moderate likelihood gain events. Besides, experiments and surveys also documented ambiguity seeking behavior, especially if ambiguity relates to low likelihood gains (e.g., Curley and Yates, 1989; Dimmock et al., 2012, 2013) or to moderate likelihood losses (e.g., Casey and Scholz, 1991; Hogarth and Kunreuther, 1985, 1989; Viscusi and Chesson, 1999). Generally, similar to findings on decision-making under risk, as formalized by Prospect Theory (Kahneman and Tversky, 1979), individual ambiguity attitudes are likely to exhibit a boundary effect, such that small likelihoods are overweighed while high likelihoods are underweighted (e.g., Abdellaoui et al., 2005, 2011). This might, for example, explain a preference for ambiguity for low likelihood gain prospects. Moreover, several studies reported a reflection effect which describes a reversal of attitudes between the outcome domain of gains to losses (see, e.g., Ho et al., 2002; Abdellaoui et al., 2005; Kothiyal et al., 2014, and those studies which we will review below).

Taken together, there is vigorous experimental evidence for the existence of a fourfold pattern of ambiguity attitudes. Individuals might prefer ambiguity to risk for gains with low likelihoods, where ambiguity aversion increases with increasing likelihoods. At the same time, individuals might be averse to ambiguity for losses which occur with low likelihoods, where ambiguity seeking increases with increasing likelihoods.

This pattern has been documented in some experimental studies which we summarize in Table C.1. These studies elicit ambiguity attitudes for the gain as well as loss domain, both for different levels of likelihoods. In Table C.1 we distinguish between two main experimental design features, i.e., whether treatment variations are implemented within or between subjects (horizontal dimension), and whether real financial incentives are employed to elicit truthful preferences (vertical dimension).

Starting with Kahn and Sarin (1988), who elicited ambiguity attitudes in the context of consumer choice experiments, the experimental tasks range from auctions over the right to insure negative outcomes and to assure positive outcomes (Di Mauro and Maffioletti, 2004), the elicitation of certainty equivalents (Abdellaoui et al., 2005), typical Ellsberg urn choice experiments (Vieider et al., 2012), and the elicitation of probability equivalents over ambiguous prospects (Baillon and Bleichrodt, 2013).

Most of these studies, however, document changes in individual ambiguity attitudes across domains, by varying outcome domains and probability levels within instead of between subjects. Only Di Mauro and Maffioletti (2004) implement choice tasks with respect to gain and loss prospects in a between-subjects design, while still varying probabilities on the subject level. Moreover, to be able to compare elicited attitudes for gain and loss prospects, the experimental design has to induce a salient reference point of wealth, with respect to which changes in wealth (gains as well as losses) are implemented. Thus, an initial endowment given to subjects in the beginning of an experiment, in particular to cover losses during the experiment, should be kept constant across domains. Among the studies discussed in Table C.1, this is the case only in Kahn and Sarin (1988) and (Baillon and Bleichrodt, 2013).

Nevertheless, Table C.1 represents reasonable experimental evidence on the existence of the particular fourfold pattern of ambiguity attitudes. Yet, we are also aware of some experiments which report (at least partially) conflicting findings with respect to this pattern. Among those studies which considered all four relevant domains, Einhorn and Hogarth (1986) as well as Budescu et al. (2002) find evidence for ambiguity neutral preferences for moderate likelihood losses. Keren and Gerritsen (1999) even report ambiguity aversion in all domains. The design features of these studies are summarized in Table C.2.

Einhorn and Hogarth (1986) and Keren and Gerritsen (1999) employ a between-subjects design for variations in outcomes and probabilities, similar to our design. However, both studies do not employ financial incentives, and also consider likelihoods that are either close to extreme (50% versus 0.1% in Einhorn and Hogarth, 1986) or generally close to moderate levels (1/3 versus 2/3 in Keren and Gerritsen, 1999).

In this study, we use a clean between-subjects design to study attitudes over prospects involving gains versus losses, which occur with either low versus moderate likelihood, respectively. We collect data from roughly 500 participants, keep endowments for participants constant across different treatments, and we control for trust and skewed beliefs about ambiguous prospects.

	Within-subjects variation			Within- and between-subjects variation	Between-subjects variation
Real incentives	**Kahn and Sarin (1988)** *Choice dimension*: direct choice between risk and ambiguity *Stimuli*: "ambiguous versus unambiguous probabilities" in consumer contexts, product choices regarding (i) warranty decisions, (ii) pharmaceutical decisions, (iii) service decisions. *Price*: $3, 5, or 7 outcomes, $5 reference outcome *Probabilities*: - *# of subjects*: 60 *Country*: USA *Notes*: reference income induced by paying 75% of subjects a fixed amount, and 25% of subjects according to their choices with outcomes below or above reference outcome	**Vieder et al. (2012)** *Choice dimension*: Choice lists for certainty equivalents over risky and uncertain prospects *Stimuli*: Ellsberg urns *Price*: +/- ETB 120 ($6), ETB 120 only endowment for loss task *Probabilities*: 12.5, 25, 37.5, 62.5, 75, 87.5% *# of subjects*: 157 *Country*: Ethiopia *Notes*: additionally study stake effects	**Baillon and Bleichrodt (2013)** *Choice dimension*: choice lists for matching probabilities *Stimuli*: variation in stock indices (home and foreign index) *Price*: +/- €10 ($14), €15 ($20) endowment *Probabilities*: 6 different uncertain events, stock variation is an element of [-100%, -0.5%], [-0.5%, 0.5%], [0.5%, ∞], and each of their complements *# of subjects*: 37 *Country*: The Netherlands *Notes*: enforced monotonicity in choice lists	**Di Mauro and Maffioletti (2004)** *Choice dimension*: bids in second price actions (market mechanism); compete for the right to insure against a loss or to assure a gain *Stimuli*: expert guess on 'best estimate' or 'probability interval' *Price*: +/- £10 ($17), £10 endowment only for loss task *Probabilities*: 3, 20, 50, 80% *# of subjects*: 116 *Country*: UK *Notes*: risky and ambiguous scenarios and different probabilities implemented within subjects; two ambiguity stimuli and gains versus losses implemented between subjects	**THIS PAPER** *Choice dimension*: direct choice, and choice list for probability equivalents *Stimuli*: 2- and 10-color Ellsberg urn *Price*: +/- €20 ($27), €20 endowment *Probabilities*: 10, 50% *# of subjects*: 289 *Country*: Germany
No incentives	**Abdellaoui et al. (2005)** *Choice dimension*: series of binary choices to elicit CPT decision weights and subjective probabilities *Stimuli*: variation in stock index (DAX) *Price*: DM 50 ($25) flat fee *Probabilities*: [-100%, -13%], [-13%, -4%], [-4%, 4%], [4%, 13%], [13%, ∞], and all contiguous union intervals *# of subjects*: 41 *Country*: Germany *Notes*: elicit CPT decision weighting functions in a two-step procedure, i.e. (i) elicit utility function, (ii) elicit certainty equivalents for bets on ambiguous events; subjective probabilities measured in terms of probability equivalents (only for gains; functionally translated for losses)	**Viscusi and Chesson (1999)** *Choice dimension*: direct choice between risky and ambiguous prospect & direct elicitation of probability equivalents *Stimuli*: conflicting expert estimates of loss probability; agreeing for risky alternative *Price*: - *Probabilities*: 5 to 95% *# of subjects*: 266 *Country*: USA *Notes*: Survey study			**Hogarth and Kunreuther (1985, 1989)** *Choice dimension*: maximum buying price (consumers) and minimum selling price (firms) *Stimuli*: 'defective product scenario'; "feel confident" (non-ambiguous case) or "experience considerable uncertainty" (ambiguous case) about probability estimates *Price*: - *Probabilities*: 1, 35, 60, 90% *# of subjects*: 113 *Country*: USA *Notes*: survey study; probability levels and roles of consumers and firms implemented between subjects

Table C.1: Literature overview - fourfold pattern of ambiguity attitudes

Notes: Cells with text printed in gray-blue refer to papers which study ambiguity attitudes in the loss domain only. Cells with text printed in bronze refer to the design of this paper; to our knowledge, there is no other experimental paper which falls into this category.

	Within-subjects variation	Within- and between-subjects variation	Between-subjects variation
Real incentives	**Budescu et al. (2002)**: find rather weak ambiguity aversion, and an insensitivity of attitudes with respect to probability level *Choice dimension*: direct response for certainty equivalents *Stimuli*: vague probabilities with width 0.3 around center values *Price*: +/- $ 5, 10, 15, $ 5 endowment *Probabilities*: 25, 50, 75% *# of subjects*: 24 *Country*: USA *Notes*: endowment not sufficient to cover all potential losses; three gain prospects and one loss prospect randomly selected for payment (hedging possible); subjects could refuse from playing any gamble and keep endowment; additionally test ambiguity attitudes with respect to outcomes		THIS PAPER
No incentives		**Einhorn and Hogarth (1986)**: find ambiguity aversion for gains with small and moderate likelihood as modal response; ambiguity aversion for low likelihood losses and ambiguity neutrality for moderate likelihood losses *Choice dimension*: direct choice between risky and ambiguous prospect *Stimuli*: 2-color Ellsberg urn, and urn with numbered balls *Price*: - *Probabilities*: 0.1 and 50% *Notes*: two probability levels implemented within subjects; gains versus losses implemented between subjects; allow for indifference *# of subjects*: 274 *Country*: USA	**Keren and Gerritsen (1999)**: find ambiguity aversion in each cluster *Choice dimension*: direct choice between risk and ambiguity (betting color) *Stimuli*: 3-color Ellsberg urn *Price*: NLG 6 ($ 6) flat fee *Probabilities*: 1/3, 2/3 *Notes*: four treatments in which subjects would win / lose if if their chosen color would match / not match the randomly drawn ball *# of subjects*: 258 *Country*: The Netherlands

Table C.2: Literature overview - different findings from fourfold pattern of ambiguity attitudes

C.2 Instructions for the treatment for gains with moderate likelihoods ($20_{.5}0$)

Welcome to the experiment and thank you for your participation!

Please do not talk to other participants of the experiment from now on.

General information on the procedure

This experiment is conducted to investigate economic decision-making. You can earn money during the experiment. It will be paid to you privately and in cash after the experiment. The entire experiment lasts about 1 hour and consists of 4 parts. At the beginning of each part you will receive detailed instructions. If you have questions after the instructions or during the experiment please raise your hand. One of the experimenters will then answer your question privately. During the experiment you will be asked to make decisions. Your own decisions will determine your payment which is a result of the following rules. While you will be making your decisions a clock will count down at the right upper corner of the screen. This provides you with an orientation about how much time you should spend on your decisions. Of course you can take more time if you need to; this might be especially likely in the beginning of the experiment. Only the information screens where no decisions need to be made will disappear after the time has run out.

Payment
In each part of the experiment your income is directly stated in Euro. Of Part I, Part II, and Part III only one part will be paid out. One participant will select which of those parts will be payoff relevant, randomly and with equal probability at the end of the experiment (after Part IV). As you do not know which part will be chosen, it is optimal for you to behave like each part was to be paid. Part IV is definitely relevant for your payment. In the beginning of the experiment you will also receive an endowment of 20 Euro. Your total income is then equal to the sum of your endowment, the income of the selected part (I, II, or III), and of Part IV.

Anonymity
We evaluate any data of the experiment only in aggregate form and never connect personal information to individual data. At the end of the experiment you have to sign a receipt for the payment. This only serves for our internal accounting.

Devices
At your place you find a pen. Please leave it on the table after the experiment.

Start
In the beginning of the experiment I ask you to choose a color, which will be your personal decision color during the experiment. You will learn for what this color is important in the following instructions.
On the first screen a list of colors will be displayed. Please mark exactly one of those colors and confirm your choice by clicking the OK-button in the lower part of the screen. All participants choose from the same list of colors. As soon as every participant has chosen his personal decision color the instructions for the first part of the experiment will be distributed.

Part I

Task
In this part you have to choose between two prospects. These prospects are described by two opaque bags, bag A and bag B. From each of these bags one chip is randomly drawn which will determine your payment as described further below. You choose whether your chip should be drawn from bag A or from bag B.

Bag *A*: Bag *A* has already been filled with exactly 100 colored chips before the experiment. These chips are either red or blue. The distribution of the colors is unknown to you: a student assistant has randomly drawn 100 chips from a bigger bag that contained far more than 100 chips - only red and blue ones. Thus, you do not know how many of the 100 chips are red or blue. If you choose bag A, you receive 20 Euro if the color of the chip that will be drawn from bag A is equal to you personal decision color, and 0 Euro if the chip has a different color.

Bag B: In a moment we will fill exactly 100 chips into bag B. Of those chips, exactly 50 are red and the remaining 50 are blue. If you choose bag B, you receive 20 Euro if the color of the chip that will be drawn from bag B is red, and 0 Euro if the chip is not red. Part I ends as soon as everyone has made his decision, you will then receive the instructions for Part II.

Payment

After the completion of Part IV the computer will randomly assign two participants as assistants. One assistant will first draw a ball from a bag filled with three balls - numbered from 1 to 3. The number of that ball determines the payoff relevant part.

If Part I is selected for payment the assistant will draw one chip from each of the bags A and B. The colors of these chips are then relevant for your payment (depending on whether you have chosen bag A or bag B). The other assistant will enter the color of the chips on his screen. Whether you have been assigned the role of an assistant will be shown to you on your screen at that point.

Part II

Task

In this part you will receive in total 9 decision problems. These will be displayed to you simultaneously on one screen. In each of these problems you choose between two prospects which we will again describe by two opaque bags.

In each of these problems you decide between bag A from Part I and a second bag, denoted by bag C. Bag C also contains exactly 100 - only red and blue - chips. How many of the chips are red and blue will be displayed on your screen.

To remind you: bag A has been randomly filled with 100 chips before the experiment. These chips are either red or blue. You do not know how many are red and how many are blue. The decision problem form Part 1 is one example for a possible decision problem in this part. Another example is illustrated in the following table:

Bag A	Bag C	Your decision
Bag A contains exactly 100 chips. You do not know how many of those are red or blue. If a chip is drawn that is of your personal decision color you receive 20 Euro. If a different chip is drawn you receive 0 Euro.	Bag C contains exactly 100 chips of which exactly 40 are red. If a red chip is drawn, you receive 20 Euro. If a different chip is drawn, you receive 0 Euro.	Bag A or Bag C

Example for choice between A and C

Your decision is not valid before you have made a choice for all decision problems and then clicked on the OK-button in the lower part of the screen. Take enough time for your decisions, as each decision can determine your payment from this part.

Payment

If Part II is selected as payoff relevant, your income from this part will be determined as follows: for each participant the computer selects randomly and with equal probability one of the 9 decision problems (i.e. each with a probability of 1/9). Each bag C will be filled with the corresponding number of red and blue chips. One assistant will draw one chip from each of these bags, and one chip from bag A, which will determine your payment as described above. If, for example, the upper decision problem is chosen and you have chosen bag C you will receive 20 Euro if the chip from this bag is red, and 0 Euro if it is not red. If you have chosen bag A you receive 20 Euro if the chip from this bag is of your personal decision color that you have chosen yourself in the beginning. Since you do not know which of the 9 decision problems will be selected for payment, it is optimal for you to behave as if each decision problem was relevant for payment.

Part III

Task

Part III consists of two periods: in each period 21 decision problems will be displayed simultaneously on your screen. In each of these problems you choose between a prospect and a safe amount of money. Within one period the prospect remains unchanged, whereas the safe amount of money is increasing with every decision problem.

<u>Period 1:</u> in the first period the prospect is given by bag B from Part I.
To remind you: bag B contains exactly 100 colored chips of which 50 are red and 50 are blue. If a red chip is drawn from the bag, you receive 20 Euro, otherwise, you receive 0 Euro. One example is illustrated in the following table:

Bag B	Amount of money	Your decision
Bag B contains exactly 100 chips of which exactly 50 are red and 50 are blue. If a red chip is drawn you receive 20 Euro. If a different chip is drawn you receive 0 Euro.	You receive 9 Euro for sure.	Bag B or Safe amount of money

Example for choice between A and C

Your decisions in period 1 are not valid before you have made a choice for all decision problems and then clicked on the OK-button in the lower part of the screen. Period 2 will start afterwards.

<u>Period 2:</u> in the second period the prospect is given by bag A from Part I (and Part II).
To remind you: bag A was randomly filled with 100 chips before the experiment; these chips are either red or blue. You do not know how many of those chips are red or blue. You receive 20 Euro if the chip that will be drawn from bag A is of your personal decision color, and 0 Euro if this chip is of a different color. The example for a decision in period 2 is analogous to the

upper example from period 1; however, you choose between bag A, instead of bag B, and the safe amount of money.

Your decisions in period 2 are again not valid before you have made a choice for all decision problems and then clicked on the OK-button in the lower part of the screen. Please note: since the safe amount of money increases continuously, you should, as soon as you have chosen the safe amount for once, do so for all remaining decisions in this period. Take enough time for your decisions, as each decision can determine your payment from this part.

Payment

If Part III is selected as payoff relevant the computer will randomly and with equal probability select one of both periods, and one of the 21 decision problems. Your decision in this problem determines your payment. If you have chosen the prospect in that particular decision problem, then, for period 1, the color of the chip from bag B determines your payment, and for period 2, the color of the chip from bag A determines your payment. Since you do not know which period and which of the 21 decision problems will be selected for payment, it is optimal for you to behave as if each decision problem was relevant for payment.

Instructions for Part IV can be received from the authors upon request.

Figure C.1: Picture of lab room and colored chips

C.3 Definition of ambiguity neutrality

$p = 0.5$	Your choice		Risky prospect with prob. q_i
Ambiguous prospect		x	0.25
		x	0.3
		x	0.35
		x	0.4
		x	0.45
	(x)	[x]	$0.5 = p$
	x		0.55
	x		0.6
	x		0.65

$p = 0.1$	Your choice		Risky prospect with prob. q_i
Ambiguous prospect		x	0.01
		x	0.04
		x	0.07
	(x)	[x]	$0.1 = p$
	x		0.13
	x		0.16
	x		0.19
	x		0.22
	x		0.25

Table C.3: Switching points for ambiguity neutrals subjects

Table C.3 demonstrates the possible decisions of ambiguity neutral subjects in the stage 2 choice lists. Because the subject is indifferent between the risky and ambiguous prospect in the decision item which is framed by dashed lines, she may choose either prospect in this choice item. Hence, switching may either occur already in that row (choice indicated by parentheses), or in the next decision item (choice indicated by square brackets).

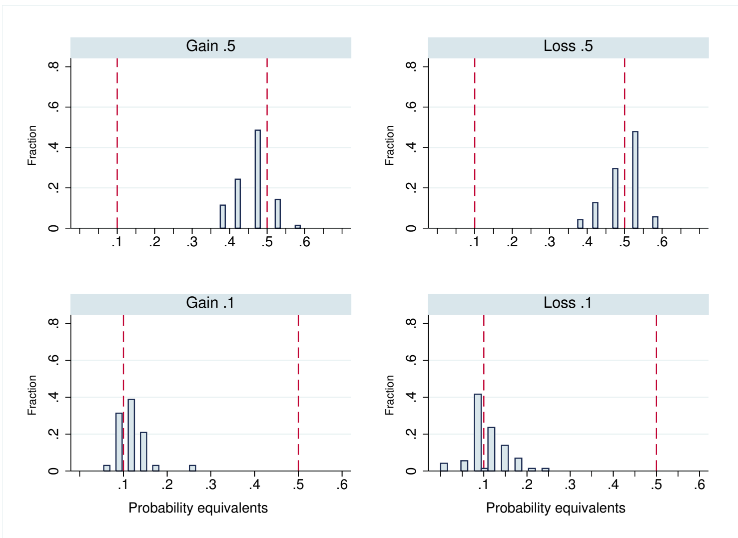


Figure C.2: Histograms of probability equivalents by treatment

C.4 Supplementary tables and figures

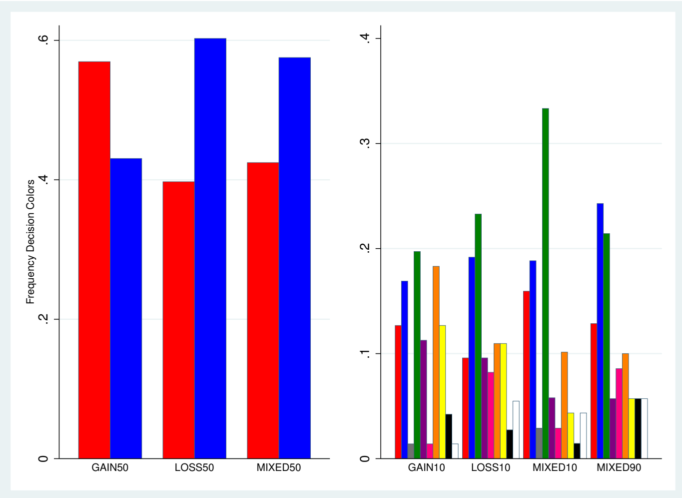


Figure C.3: Frequencies of chosen decision colors (all observations included)

	Treatment		
	$20_{.5}0$	$(-20)_{.5}0$	$(-10)_{.5}10$
Red	56.9%	39.7%	42.5%
Blue	43.1%	60.3%	57.5%

	Treatment			
	$20_{.1}0$	$(-20)_{.1}0$	$(-10)_{.1}10$	$10_{.1}(-10)$
Red	12.7%	9.6%	15.9%	12.9%
Blue	16.9%	19.2%	18.8%	24.3%
Grey	1.4%	0.0%	2.9%	0.0%
Green	19.7%	23.3%	33.3%	21.4%
Purple	11.3%	9.6%	5.8%	5.7%
Pink	1.4%	8.2%	2.9%	8.6%
Orange	18.3%	11.0%	10.1%	10.0%
Yellow	12.7%	11.0%	4.3%	5.7%
Black	4.2%	2.7%	1.4%	5.7%
White	1.4%	5.5%	4.3%	5.7%

Table C.4: Frequencies of chosen decision colors (all observations included)

Treatment	# obs. Stage 1 (Stage 2)	Stage 1: ambiguous choices (%)	Stage 2: probability equivalent[a]	Consistent between stage 1 and 2[b] (%)
$20_{.5}0$	70	37.1 AA**	.48 AA***	72.9
$(-20)_{.5}0$	71	49.3 (AA)	.53 (AA)	88.7
$20_{.1}0$	67	55.2 (AS)	.12 AS***	82.1
$(-20)_{.1}0$	72	62.5 AS**	.09 (AS)	79.2

Table C.5: Ambiguity attitudes for restricted samples in Part 1 and Part 2

Notes: [a]: median; [b]: same choice made in Part 1 and the respective decision item in Part 2; direction of effect: AA=ambiguity averse; AS=ambiguity seeking; *,**,*** denote significance at the 10%, 5%, and 1% level; Part 1: two-sided binomial test against p=0.5/0.1; Part 2: two-sided t-test against probability equivalent=0.5/0.1. Data of subjects who violate consistency criteria in Part 2 excluded in Part 1 and 2.

Treatment	# obs. Stage 1 (Stage 2)	Stage 1: ambiguous choices (%)	Stage 2: probability equivalent[a]	*Consistent between stage 1 and 2[b] (%)*
$20_{.5}0$	51	17.6 AA***	.48 AA***	*100*
$(-20)_{.5}0$	63	47.6 (AA)	.53 (AA)	*100*
$20_{.1}0$	55	60.0 (AS)	.12 AS***	*100*
$(-20)_{.1}0$	57	56.1 (AS)	.09 (AS)	*100*

Table C.6: Ambiguity attitudes for consistent subjects

Notes: [a]: median; [b]: same choice made in Part 1 and the respective decision item in Part 2; direction of effect: AA=ambiguity averse; AS=ambiguity seeking; *,**,*** denote significance at the 10%, 5%, and 1% level; Part 1: two-sided binomial test against p=0.5/0.1; Part 2: two-sided t-test against probability equivalent=0.5/0.1. Data of subjects who violate consistency criteria in Part 2 and whose choices in Part 1 and Part 2 are not consistent excluded in Part 1 and 2.

C.5 Consistency

Inconsistencies might be caused by indifferent preferences in stage 1, in which case choosing different prospects in stage 1 and the corresponding decision item in stage 2 is rational. To further test whether inconsistencies might bias our overall results we distinguish between consistent and inconsistent subjects in Figure C.4, and picture their average stage 1 choices in the upper row, and median probability equivalents from stage 2 in the lower row. Differences between consistent and inconsistent subjects become most obvious with respect to direct choices, and in particular in treatment $20_{.5}0$. Nearly 90% of inconsistent subjects choose the ambiguous urn, in contrast to only 18% of consistent subjects. Similarly, in treatment $(-20)_{.1}0$, 87% of inconsistent subjects are ambiguity seeking, compared to only 56% of consistent subjects. Both differences are significant (Fisher exact tests, p-values <0.01 for $20_{0.5}0$, and 0.037 for $(-20)_{.1}0$). That is, inconsistent subjects on average deviate from the fourfold pattern of attitudes reported for non-neutrals in section 3.4.2. In contrast, we do not find stark differences in terms of median probability equivalents. Only for low likelihood loss prospects, inconsistent subjects are significantly more ambiguity seeking, which indicates the same bias as stage 1 choices (Wilcoxon rank-sum test, p-value 0.037).

Given that differences between inconsistent and consistent subjects are particularly apparent for direct choices in which we cannot identify ambiguity neutrality, inconsistencies and ambiguity neutrality might be positively correlated. Such a correlation, however, might confound our finding reported in section 3.4.2, namely that a fourfold pattern unfolds if we abstract from the ambiguity neutrals: deviations from the fourfold pattern might be due to subjects who exhibit inconsistent rather than neutral preferences.

We argue that this is not the case, by comparing the frequency of ambiguity neutrals between consistent and inconsistent subjects. Numbers are reported in Table C.7. Except for standard prospects with moderate likelihood gains, the fraction of consistent subjects

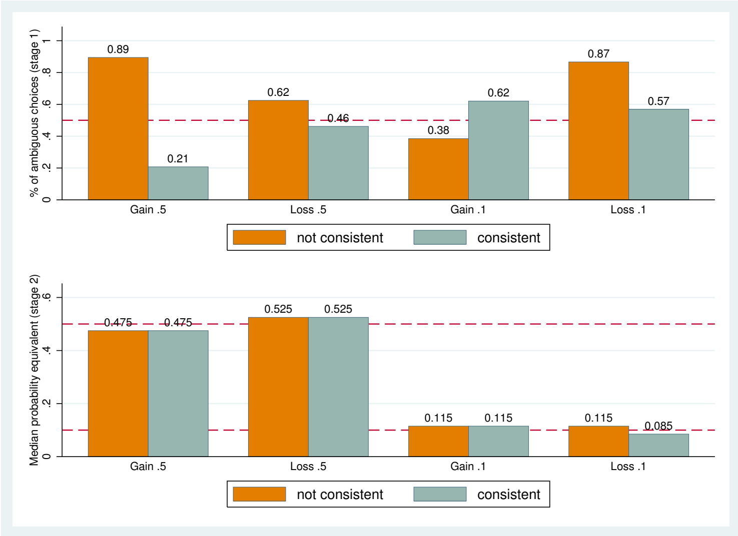


Figure C.4: Ambiguity attitudes by treatment and consistency

Notes: Numbers of observations denoted (# of cons. subjects, # of incons. subjects) are as follows:
Stage 1: (19, 53) for $20_{.5}0$, (8, 65) for $(-20)_{.5}0$, (13, 58) for $20_{.1}0$, and (15, 58) for $(-20)_{.1}0$.
Stage 2: (19, 51) for $20_{.5}0$, (8, 63) for $(-20)_{.5}0$, (12, 55) for $20_{.1}0$, and (15, 57) for $(-20)_{.1}0$.

does not significantly differ between ambiguity neutrals and non-neutrals (Fisher exact tests, p-values 0.028 for $20_{.5}0$, 0.673 for $(-20)_{.5}0$, 0.741 for $20_{.1}0$, and 0.552 for $(-20)_{.1}0$). Yet, ambiguity aversion in treatment $20_{.5}0$ appears to be the only attitude which is robust against any subsample and elicitation task. Thus, a hidden fraction of inconsistent choices is rather unlikely to confound the revelation of the fourfold pattern by abstracting from ambiguity neutrality.

	Treatment						
	$20_{.5}0$	$(-20)_{.5}0$	$20_{.1}0$	$(-20)_{.1}0$	$(-10)_{.5}10$	$(-10)_{.1}10$	$10_{.1}(-10)$
Including ambiguity neutrals	72.9%*	88.7%*	82.1%*	79.2%*	72.1%*	74.6%*	78.3%*
Excluding ambiguity neutrals	88.5%*	93.8%*	80.0%*	75.0%*	75.0%	63.6%	92.3%*
Ambiguity neutrals	63.6%	87.3%*	83.0%*	81.3%*	71.4%*	80.0%*	75.0%*

Table C.7: Consistency of ambiguity attitude

Notes: Entries report percentages of subjects who make consistent choice in the identical choice item in stage 1 and stage 2 of the experiment. * indicates that the percentage is larger at the 5% significance level than expected under random choices (50% consistency).

Appendix D

Social Anchor Effects in Decision-Making under Ambiguity

D.1 Instructions for the GAIN-IND and GAIN-PEER treatments

The instruction for Part I and with respect to general information about the experiment are identical in IND and PEER treatments. Instructions of IND and PEER treatments only differ in a few sentences in Part II. Sentences or words which are included in the PEER but not in the IND treatments are written in blue text color; sentences/words which are included in the IND but not in the PEER treatments are written in green text color.

Welcome to the experiment and thank you for your participation!
Please do not talk to other participants of the experiment from now on.

General information on the procedure
This experiment is conducted to investigate economic decision-making. You can earn money during the experiment. It will be paid to you privately and in cash after the experiment. The entire experiment lasts about 1 hour and consists of 3 parts. At the beginning of each part you will receive detailed instructions. If you have questions after the instructions or during the experiment please raise your hand. One of the experimenters will then answer your question privately. During the experiment you will be asked to make decisions. In the course of the experiment it is possible that other participants will get to know your decisions from a previous part of the experiment. In this case, this will happen anonymously: it is neither possible to allocate your decisions to your seat number or your person, nor to draw conclusions on your payment. Only your own decisions determine your payment, which is a result of the following rules.

Payment

In each part of the experiment your income is directly stated in Euro. Of Part I and Part II only one part will be paid out. Which of both parts will be relevant for payments will be chosen randomly and with equal probability by the computer at the end of the experiment (after Part III). Since you do not know which of the parts (Part I or Part II) will be selected, it is optimal for you to behave as if each part was to be paid out. Part III is definitely relevant for your payment.

In the beginning of the experiment you will also receive an endowment of 10 Euro. Your total income is then given by the sum of your credit, the income of Part III, and the part (I or II) which was selected for payment.

Anonymity

I evaluate all the data of the experiment only in aggregate form and never connect personal information to the data of the experiment. At the end of the experiment you have to sign a receipt for the payment. This only serves for our internal accounting.

Devices

At your place you will find a pen. Please leave it on the table after the experiment.

Start

In the beginning of the experiment I ask you to choose a color, which will be your personal decision color during the experiment. You will learn for what this color is important in the following instructions.

On the first screen a list of colors will be displayed. Please mark exactly one of those colors and confirm your choice by clicking the OK-button in the lower part of the screen. All participants choose from the same list of colors. As soon as every participant has chosen his personal decision color the instructions for the first part of the experiment will be distributed.

Part I

Task

In this part you receive 21 decision problems. These will be displayed simultaneously on your screen. In each of the decision problem you choose between two lotteries. I describe these lotteries with two opaque bags, bag A and bag B. In the end of the experiment one chip will be drawn randomly from each of these bags. This chip will determine your payment, as described further below. Thus, you choose whether your chip should be drawn from bag A or from bag B.

<u>Bag A</u>: Bag A was already filled with exactly 100 colored chips before the experiment. Those chips are either red or blue. The distribution of those colors is unknown to you: a student assistant has randomly drawn 100 chips from a bigger bag that contained far more than 100 chips - only red and blue ones. Thus, you do not know how many of the 100 chips are red or blue.

If you choose bag A, you receive 10 Euro if the color of the chip that will be drawn from bag A is of your personal decision color, and 0 Euro if the chip is of a different color.

Bag B: Bag B also contains in total 100 chips which are either red or blue. How many of the chips are red and blue will be displayed on your screen. If you choose bag B, you receive 10 Euro if the color of the chip that will be drawn from bag B is red, and 0 Euro if the chip is not red.
An example of one decision problem is illustrated in the following table:

Bag A	Bag B	Your decision
Bag A contains exactly 100 chips. You do not know how many of those are red or blue. If a chip is drawn that is of your personal decision color you receive 10 Euro. If a different chip is drawn you receive 0 Euro.	Bag B contains exactly 100 chips of which exactly 16 are red. If a red chip is drawn, you receive 10 Euro. If a different chip is drawn, you receive 0 Euro.	Bag A or Bag B

Example for choice between A and B

Your decision is not valid before you have made a choice for all decision problems and then clicked on the OK-button in the lower part of the screen. Take enough time for your decisions, as each decision can determine your payment from this part.

Payment
After the completion of Part III the computer will randomly choose whether Part I or Part II is relevant for your payment. Both parts will be selected with the same probability. If Part I is relevant for your payment, the computer will randomly and with equal probability select one of the 21 decision problems. Your decision in this problem determines your payment.
In addition, the computer will randomly choose two participants as assistants. For bags B of decision problems 9 to 17 an opaque bag will be filled with the corresponding number of red and blue chips. Assistant no. 1 will then draw one chip from each of those bags, and one chip from bag A, which will determine your payment. Assistant no. 2 will enter the colors of the drawn chips on his screen. In the interest of time, for the remaining bags B of decision problems 1 to 8 and 18 to 21 the computer will randomly draw a chip, corresponding to the respective distribution of red and blue chips.
If, for example, the above decision problem is chosen for you and you have chosen bag B, then you receive 10 Euro if the chip from this bag is red and 0 Euro otherwise. If you have chosen bag A in this decision problem, then you receive 10 Euro if the chip is of your personal decision color that you have chosen in the beginning of the experiment yourself. Since you do not know which of the 21 decision problems will be selected for your payment, it is optimal for you to make your choices as if each decision problem was relevant for payment.

Part II

Task

[PEER] In the beginning of Part II you are randomly assigned to another participant of the experiment, with who you will form a group. Your group number will be displayed on your screen in the beginning. In this part you have the opportunity to reconsider your decision from Part I. Therefore, the 21 decision problems will again be displayed simultaneously on your screen. Simultaneously, you see the decisions that you have made in Part I [PEER] and the decisions that your group member has made in Part I. Part II ends again after you have made all decisions. In this part you should also take enough time for your decisions, as every decision can determine your payment for this part of the experiment.

Payment

In the end of the experiment, the computer will randomly choose for each participant whether Part I or Part II is relevant for payment. [PEER] For each group holds that both members are paid for different parts: if for you part I is paid, then for your group member part II is Paid. If Part II is payoff relevant for you, then your group member is paid for Part I. Part I and Part II are selected with equal probability for every participant. Then, the computer will randomly and with equal probability choose one of the 21 decision problems for each [IND] participant [PEER] group. Your decision in this problem of your respective part determines your payment. For bags B of decision problems 9 to 17 an opaque bag will be filled with the corresponding number of red and blue chips. Assistant no. 1 will then draw one chip from each of those bags, and one chip from bag A, which will determine your payment. Assistant no. 2 will enter the colors of the drawn chips on his screen. In the interest of time, for the remaining bags B of decision problems 1 to 8 and 18 to 21 the computer will randomly draw a chip, corresponding to the respective distribution of red and blue chips.
Since you do not know which of the 21 decision problems will be selected for payment, it is optimal for you to behave as if each decision problem was relevant for payment.

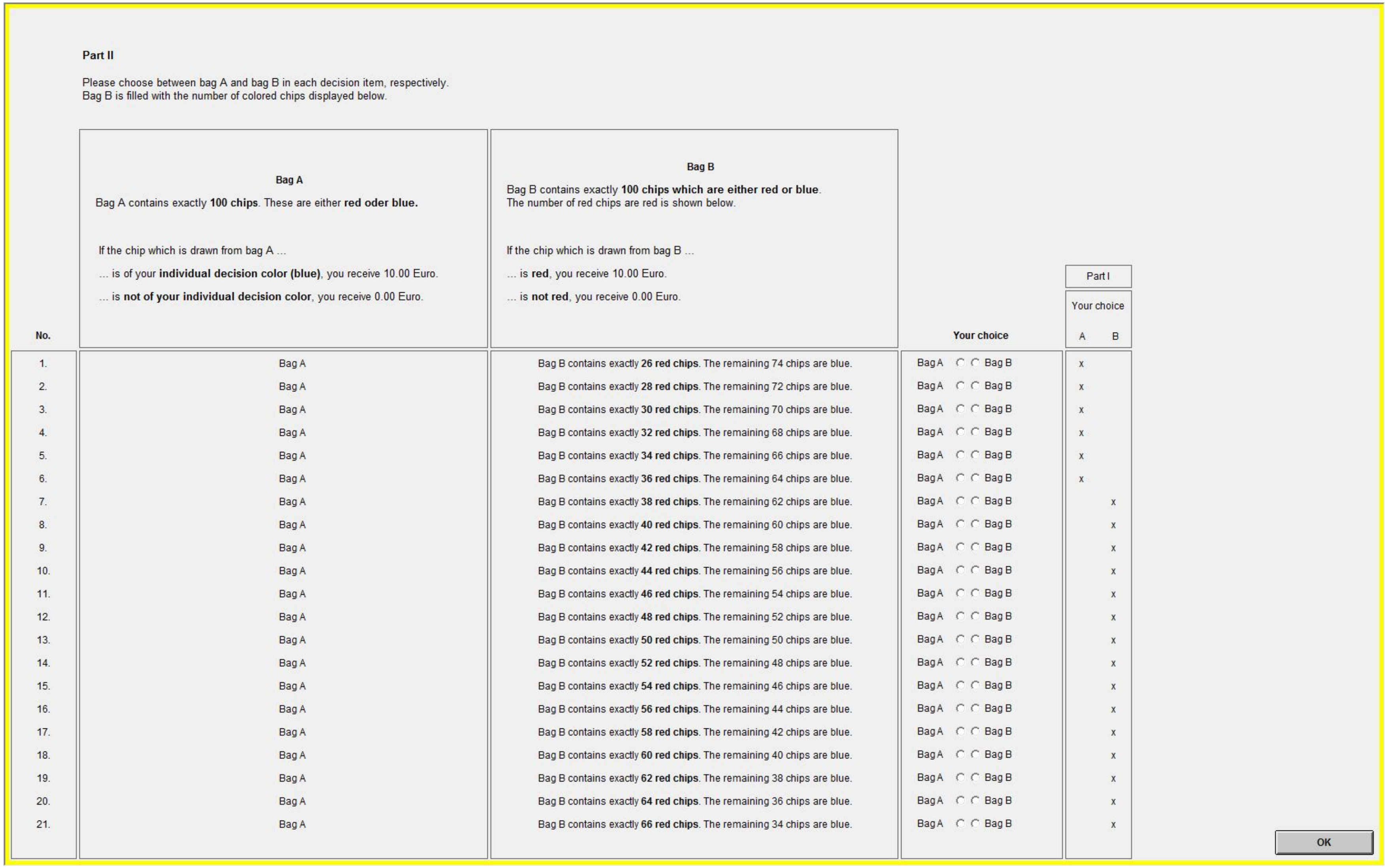
Figure D.1: Picture of lab room

D.2 Supplementary tables and figures

	Part 1		Part 2	
Treatment	t-test	signrank test	t-test	signrank test
GAIN-IND	0.000	0.000	0.000	0.000
LOSS-IND	0.000	0.001	0.010	0.011
GAIN-PEER	0.000	0.000	0.000	0.000
Group 1	0.000	0.000	0.000	0.000
Group 2	0.081	0.087	0.020	0.031
LOSS-PEER	0.020	0.039	0.046	0.168
Group 1	0.701	0.735	1.000	0.569
Group 2	0.004	0.005	0.019	0.036

Notes: two-sided t-test and Wilcoxon sign rank test; both test null hypothesis that probability equivalent is equal to 0.5 (ambiguity neutrality). Median probability equivalents are given in Table 4.2 in section 4.3.2.

Table D.1: P-values for ambiguity attitudes



Part II

Please choose between bag A and bag B in each decision item, respectively.
Bag B is filled with the number of colored chips displayed below.

No.	Bag A	Bag B	Your choice	Part I – Your choice A	Part I – Your choice B
	Bag A Bag A contains exactly **100 chips**. These are either **red oder blue.** If the chip which is drawn from bag A ... ... is of your **individual decision color (blue)**, you receive 10.00 Euro. ... is **not of your individual decision color**, you receive 0.00 Euro.	**Bag B** Bag B contains exactly **100 chips which are either red or blue**. The number of red chips are red is shown below. If the chip which is drawn from bag B ... ... is **red**, you receive 10.00 Euro. ... is **not red**, you receive 0.00 Euro.			
1.	Bag A	Bag B contains exactly **26 red chips**. The remaining 74 chips are blue.	Bag A ◯ ◯ Bag B	x	
2.	Bag A	Bag B contains exactly **28 red chips**. The remaining 72 chips are blue.	Bag A ◯ ◯ Bag B	x	
3.	Bag A	Bag B contains exactly **30 red chips**. The remaining 70 chips are blue.	Bag A ◯ ◯ Bag B	x	
4.	Bag A	Bag B contains exactly **32 red chips**. The remaining 68 chips are blue.	Bag A ◯ ◯ Bag B	x	
5.	Bag A	Bag B contains exactly **34 red chips**. The remaining 66 chips are blue.	Bag A ◯ ◯ Bag B	x	
6.	Bag A	Bag B contains exactly **36 red chips**. The remaining 64 chips are blue.	Bag A ◯ ◯ Bag B	x	
7.	Bag A	Bag B contains exactly **38 red chips**. The remaining 62 chips are blue.	Bag A ◯ ◯ Bag B		x
8.	Bag A	Bag B contains exactly **40 red chips**. The remaining 60 chips are blue.	Bag A ◯ ◯ Bag B		x
9.	Bag A	Bag B contains exactly **42 red chips**. The remaining 58 chips are blue.	Bag A ◯ ◯ Bag B		x
10.	Bag A	Bag B contains exactly **44 red chips**. The remaining 56 chips are blue.	Bag A ◯ ◯ Bag B		x
11.	Bag A	Bag B contains exactly **46 red chips**. The remaining 54 chips are blue.	Bag A ◯ ◯ Bag B		x
12.	Bag A	Bag B contains exactly **48 red chips**. The remaining 52 chips are blue.	Bag A ◯ ◯ Bag B		x
13.	Bag A	Bag B contains exactly **50 red chips**. The remaining 50 chips are blue.	Bag A ◯ ◯ Bag B		x
14.	Bag A	Bag B contains exactly **52 red chips**. The remaining 48 chips are blue.	Bag A ◯ ◯ Bag B		x
15.	Bag A	Bag B contains exactly **54 red chips**. The remaining 46 chips are blue.	Bag A ◯ ◯ Bag B		x
16.	Bag A	Bag B contains exactly **56 red chips**. The remaining 44 chips are blue.	Bag A ◯ ◯ Bag B		x
17.	Bag A	Bag B contains exactly **58 red chips**. The remaining 42 chips are blue.	Bag A ◯ ◯ Bag B		x
18.	Bag A	Bag B contains exactly **60 red chips**. The remaining 40 chips are blue.	Bag A ◯ ◯ Bag B		x
19.	Bag A	Bag B contains exactly **62 red chips**. The remaining 38 chips are blue.	Bag A ◯ ◯ Bag B		x
20.	Bag A	Bag B contains exactly **64 red chips**. The remaining 36 chips are blue.	Bag A ◯ ◯ Bag B		x
21.	Bag A	Bag B contains exactly **66 red chips**. The remaining 34 chips are blue.	Bag A ◯ ◯ Bag B		x

OK

Figure D.2: Screenshot: Part 2 – individual condition (IND)

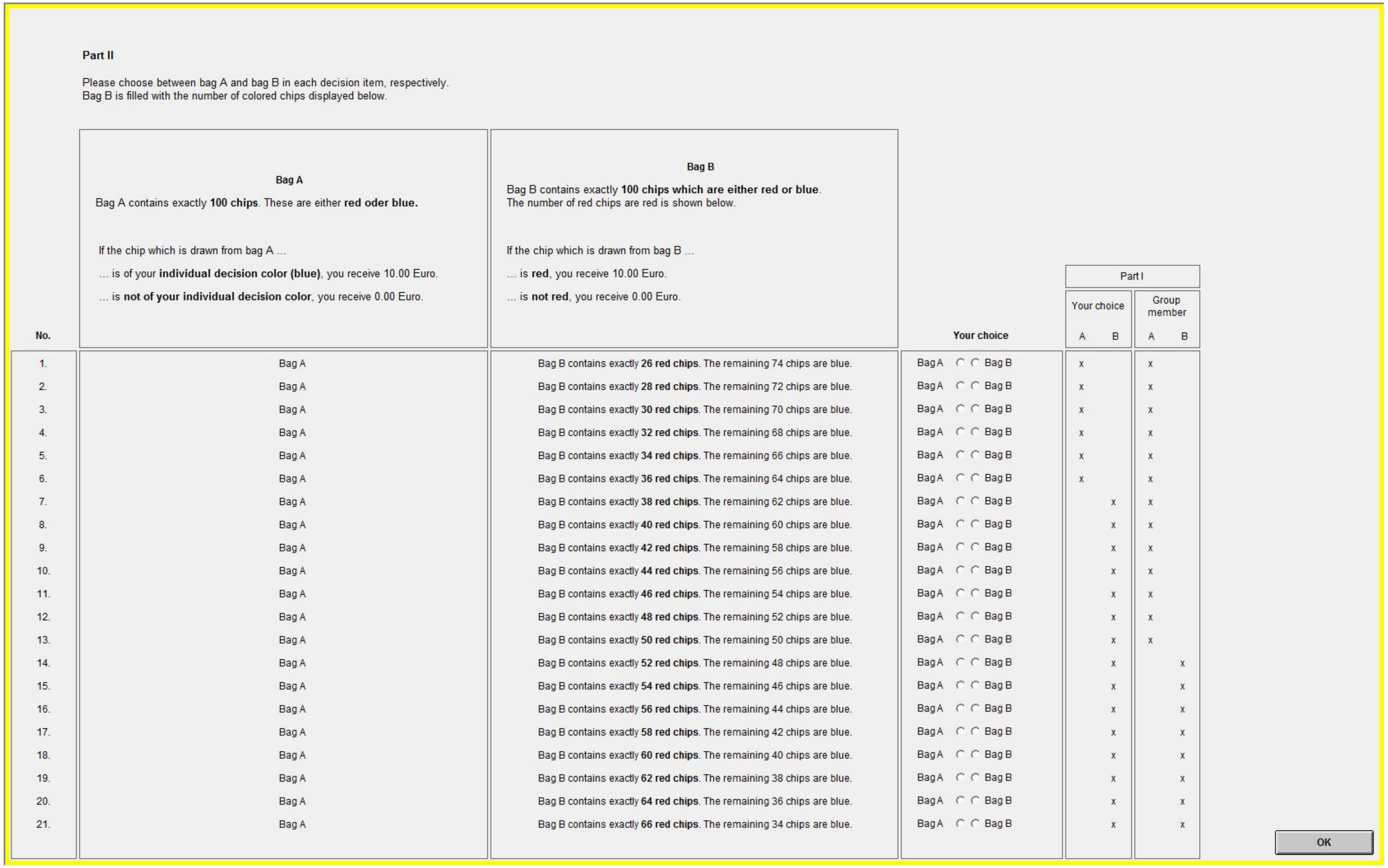


Figure D.3: Screenshot: Part 2 – social feedback (PEER)

	No change		Change			Whole sample		
	Part 1 / 2	N	Part 1	Part 2	N	Part 1	Part 2	N
GAIN-IND	0.46	20	0.48	0.45	15	0.47	0.46	35
LOSS-IND	0.50	17	0.45	0.47	18	0.47	0.49	35
GAIN-PEER	0.46	14	0.44	0.44	22	0.45	0.45	36
Group 1	0.44	6	0.40	0.43	13	0.41	0.43	19
Group 2	0.48	8	0.49	0.47	9	0.49	0.48	17
LOSS-PEER	0.48	21	0.48	0.50	17	0.48	0.49	38
Group 1	0.49	10	0.51	0.51	9	0.50	0.50	19
Group 2	0.46	11	0.44	0.49	8	0.45	0.47	19

Table D.2: Average q_{P1} and q_{P2} by changes in probability equivalents

Part 1		GAIN vs. LOSS	GAIN-IND vs. LOSS-IND	GAIN-PEER vs. LOSS-PEER
χ^2-test	Aggregate	0.000	0.000	0.000
	Group 1	0.000	–	0.003
	Group 2	0.000	–	0.021
Fisher exact test	Aggregate	0.000	0.000	0.000
	Group 1	0.000	–	0.008
	Group 2	0.000	–	0.042
Part 2		GAIN vs. LOSS	GAIN-IND vs. LOSS-IND	GAIN-PEER vs. LOSS-PEER
χ^2-test	Aggregate	0.000	0.000	0.003
	Group 1	0.009	–	0.008
	Group 2	0.000	–	0.095
Fisher exact test	Aggregate	0.000	0.000	0.004
	Group 1	0.017	–	0.019
	Group 2	0.000	–	0.181

Notes: χ^2-test and two-sided Fisher exact test to test for differences between the composition of ambiguity averse, neutral and seeking subjects between GAIN and LOSS treatments.

Table D.3: P-values for differences in ambiguity attitudes in GAIN vs. LOSS

Part 1		IND vs. PEER	GAIN-IND vs. GAIN-PEER	LOSS-IND vs. LOSS-PEER
rank-sum test	Aggregate	0.748	0.137	0.435
	Group 1	0.346	0.037	0.672
	Group 2	0.513	1.000	0.472
χ^2-test	Aggregate	0.947	0.351	0.473
	Group 1	0.897	0.969	0.858
	Group 2	0.978	0.244	0.101
Fisher exact test	Aggregate	1.000	0.514	0.625
	Group 1	1.000	1.000	1.000
	Group 2	1.000	0.438	0.182
Part 2		IND vs. PEER	GAIN-IND vs. GAIN-PEER	LOSS-IND vs. LOSS-PEER
rank-sum test	Aggregate	0.979	0.476	0.403
	Group 1	0.249	0.056	0.673
	Group 2	0.160	0.194	0.531
χ^2-test	Aggregate	0.935	0.351	0.377
	Group 1	0.506	0.132	0.842
	Group 2	0.622	0.007	0.083
Fisher exact test	Aggregate	1.000	0.514	0.478
	Group 1	0.604	0.180	1.000
	Group 2	0.641	0.018	0.128

Notes: Wilcoxon rank-sum test to test for differences in the distribution of q between IND and PEER treatments; χ^2-test and two-sided Fisher exact test to test for differences between the composition of ambiguity averse and seeking subjects between IND and PEER treatments.

Table D.4: P-values for differences in ambiguity attitudes in IND vs. PEER

Treatment	χ^2-test	Fisher exact test
All	0.089	0.126
GAIN	0.405	0.518
LOSS	0.029	0.049
IND	0.005	0.007
PEER	0.701	0.814
GAIN-IND	0.167	0.292
LOSS-IND	0.008	0.012
GAIN-PEER	0.810	1.000
Group 1	0.130	0.316
Group 2	0.402	0.620
LOSS-PEER	0.635	0.744
Group 1	0.809	1.000
Group 2	0.243	0.338

Notes: χ^2-test and two-sided Fisher exact test to test for differences in the likelihood to change between Part 1 and Part 2 between ambiguity averse and ambiguity seeking subjects.

Table D.5: P-values for differences in frequencies of change across ambiguity attitudes

	GAIN-IND		LOSS-IND		GAIN-PEER		LOSS-PEER		
Predictions model (1)	0.43		0.51		0.62		0.45		
True values	*0.43*		*0.51*		*0.61*		*0.45*		
	AA	AS	AA	AS	AA	AS	AA	AS	
Predictions model (2)	0.42	0.55	0.31	0.61	0.60	0.70	0.30	0.54	
Predictions model (3)	0.39	0.76	0.18	0.67	0.62	0.58	0.40	0.48	
True values	*0.39*	*0.75*	*0.18*	*0.67*	*0.59*	*0.69*	*0.40*	*0.48*	
					less AA	more AA	same AA	less AA	more AA
Predictions model (4)	0.43		0.51		0.53	0.68	0.15	0.44	0.60
	0.42	0.56	0.46	0.54					
Predictions model (5)	0.43		0.51		0.53	0.68	0.15	0.43	0.60
	0.41	0.59	0.32	0.60					
Predictions model (6)	0.43		0.51		0.53	0.68	0.14	0.44	0.60
	0.39	0.76	0.18	0.67					
True values	*0.43*		*0.51*		*0.53*	*0.68*	*0.14*	*0.44*	*0.60*
	0.39	*0.75*	*0.18*	*0.67*					

Notes: Predicted values for the likelihood to change based on models (1)-(4) from Tables 4.4 and 4.5.

Table D.6: Model predictions for likelihood to change

Appendix E

Team Reasoning as a Guide to Coordination

E.1 Instructions for ONE-L

Welcome to the experiment and thank you for your participation!
Please do not talk to other participants of the experiment from now on.

General information on the procedure

This experiment is conducted to investigate decision-making. You can earn money during the experiment. It will be paid to you privately and in cash after the experiment. The entire experiment lasts about 1 hour and consists of 3 parts. At the beginning of each part you will receive detailed instructions. If you have questions after the instructions or during the experiment please raise your hand. One of the experimenters will then answer your question in private. During the experiment you will be asked to make decisions. You will partly interact with other participants, i.e. your own decisions, as well as the decisions of other participants, may determine your earnings. These results from the rules explained in the following. While you make your decisions, you will see a clock running down in the right top corner of your screen. This provides you with some orientation, how much time you should need for your decision. Of course you can also exceed this time, if you need more time for your decision. Especially at the beginning, this may often be the case. Only the information screens, in which no decisions are to be made, will vanish after the time passed out.

Payment

In each part of the experiment your income is directly stated in Euro. Of part I and part II only one of the parts will be paid out. Which of the two parts will be paid out will be determined randomly and with equal probability by the computer at the end of the experiment (after part III). Your total income is then the sum of your income in part III, and the drawn part (I or II). For your punctual arrival you get 4 Euro in addition to the income you can receive during the experiment. In the beginning of the experiment you will also receive an endowment of 10 Euro.

Your total income is then given by the sum of your credit, the income of Part III, and the part (I or II) which was selected for payment.

Anonymity
We evaluate all the data of the experiment only in aggregate form and never connect personal information to the data of the experiment. At the end of the experiment you have to sign a receipt for the payment. This only serves for our internal accounting.

Devices
At your place you will find a pen. Please leave it on the table after the experiment.

Part I

Task
At the beginning of Part I, all participants are divided into teams of two people, which we denote by person A and person B. The allocation is randomly determined by the computer. It will be displayed on the screen if you are person A or person B. You remain member of the same team throughout Part I and Part II. You will not get to know the identity of your team partner at any time of the experiment. Part I consists of five identical rounds. In each round, exactly one decision is to be made. Your decision together with the decision of your team partner will influence your payout as well as your partner's payout. Task In each round, you can choose between option *red* and option blue. Also, your team partner chooses between option *red* and *blue* and you both make your decision at the same time. The income for you and your team partner is as follows:

		Decision of Person B: Red	Decision of Person B: Blue
Decision of Person A	Red	Person A receives € 5 Person B receives € 5	Person A receives € 0 Person B receives € 0
	Blue	Person A receives € 0 Person B receives € 0	Person A receives € 4 Person B receives € 4

For example: Person A selects red: then you and your team partner both receive 5 Euro if person B opts for *red* as well. If person B decides for *blue* in this case, you both get 0 Euro. If Person A selects blue, then get you and your team partner both get 0 Euro if person B chooses red; you both receive 4 Euro if person B chooses *blue* as well.
After both team partners have made their decision in the specific round, you will learn which option your team partner has chosen, and how high your income is in this round, if this round would be selected for payout. Then the next round begins. Once round 5 is completed, Part I ends and the instructions for Part II will be handed out.
If Part I is drawn as the payout relevant part at the end of the experiment (i.e. after the end of Part III), the computer again randomly selects for each team one of the 5 rounds with equal

probability ($1/5 = 0.2 = 20\%$). The amount you have achieved in this selected round, will then be paid out to you in cash. You know at the end of the experiment, whether Part I is payout relevant and if so, what round was selected for you and your team partner. Please note: Since you do not know which round is selected, you should devote the same attention to your decisions in each round.

Part II

In Part II, you are again matched with the same team partner from Part I to form a two-person team. As in Part I you are still person A or person B, and this will again be displayed to you on the screen. Part II consists of one round, that is, you make only one decision which determines, together with the decision of your team partner, your own income and the income of your team partner from this part.

Task
Person A and Person B again decide between option *red* and option *blue* and both take their decision simultaneously.
If you are person A, you will have to roll a virtual dice (by the help of the computer) before each person takes his decision. All persons A roll independently of all other persons A in the experiment. The dice's result, can affect the choice options of person A looks as follows. For each person A it holds:

- If Person A rolls a 6, then the decision of that person A is set to blue.
- If Person A rolls a number other than 6 (1, 2, 3, 4, or 5), then the decision of person A is not affected.

The probability that the decision of person A is set to blue, is hence 1/6th. Note that the dice roll only influences the choice of person A and not the decision of person B. Additionally Person B does not learn at any point in time what person A of his team has thrown. So you do not know if person A can freely choose or if he/she is set to blue.
You will make your decision after every person A has rolled the dice and is informed about its outcome. Your and your team partner's income is (as in Part I) as follows:

		Decision of Person B: Red	Decision of Person B: Blue
Decision of Person A	Red	Person A receives € 5 Person B receives € 5	Person A receives € 0 Person B receives € 0
	Blue	Person A receives € 0 Person B receives € 0	Person A receives € 4 Person B receives € 4

For example: If both people ultimately opted *red* (or blue), then both receive 5 Euro (or 4 Euro). If one person has *red* and his team partner has *blue* (or vice versa), both receive 0 Euro.

After all participants have made their decisions, you are informed about your income from this part of the experiment – in case this part will be drawn at the end as payout relevant. After that ends Part II.

Part III

In this part you will only take individual decisions. Hence, your own decisions do not affect the income of other participants and your income is completely independent of the decisions of other participants.

Task

In this part, 16 decision problems are presented to you on the screen. In each of these problems, you can choose between a lottery A and a safe amount of money, which we denote by lottery B. A lottery will remain basically unchanged, only the safe amount of money from lottery B increases with each additional decision problem. Since this safe amount of money is continually increasing, you should as soon as you have decided once for lottery B, do this for all of the following decision problems. An example for such a decision problem is presented in the following table:

Your decisions are only valid if you have made a selection for all of the problems and then clicked on the OK button at the bottom of the screen. Take enough time for your decisions because each can determine your payoff from this part.

After you have taken all the decisions, your earnings from Part III is determined as follows: The computer randomly selects with equal probability one of the 16 decision problems for each participant. If you would have selected lottery A in this case, the computer will simulate it and you receive the appropriate result. If you have opted for the safe amount of money from lottery B, you get this.

For example: Assume that the computer randomly selects the above decision problem, and you preferred lottery A. Then you either receive 5 Euro (with probability 5/6) or 0 Euro (with probability 1/6) as your payment for this part of the experiment. If you have opted for lottery B, you obtain 3 Euro with certainty.

After Part III, the experiment ends. We then ask you to complete a few questions about yourself honestly and completely. Once all participants have finished answering these questions, we will call you out one by one in random order based on your subscriber. Your earnings will be paid out privately and in cash.

After the completion of Part III the computer will randomly choose whether Part I or Part II is relevant for your payment. Both parts will be selected with the same probability.

E.2 Supplementary tables

	Player B		Player A		Player A & B	
Treatment	Risk seeking	Risk averse	Risk seeking	Risk averse	Risk seeking	Risk averse
ONE-H	0.00 %	37.50 %	50.00 %	55.56 %	33.33 %	44.00 %
N	2	16	4	9	6	25
# *red* choices	0	6	2	5	2	11
ONE-L	50.00 %	86.67 %	100.00 %	75.00 %	80.00 %	81.48 %
N	2	15	3	12	5	27
# *red* choices	1	13	3	9	4	22
TWO-L	100.00 %	62.50 %	.	66.67 %	100.00 %	64.71 %
N	1	8	0	9	1	17
# *red* choices	1	5	0	6	1	11

Notes: Enforced decisions are excluded. A subject is classified as risk averse if his certainty equivalent is smaller than the expected value of respective lottery used in the choice list of Part 3 of the experiment.

Table E.1: Frequency of *red* choices by treatment, role and risk attitude

Bibliography

Abdellaoui, M., A. Baillon, L. Placido, and P. P. Wakker (2011). The rich domain of uncertainty: Source functions and their experimental implementation. *The American Economic Review 101*(2), 695–723.

Abdellaoui, M., F. Vossmann, and M. Weber (2005). Choice-based elicitation and decomposition of decision weights for gains and losses under uncertainty. *Management Science 51*(9), 1384–1399.

Ahern, K. R., R. Duchin, and T. Shumway (forthcoming). Peer effects in risk aversion and trust. *Review of Financial Studies*.

Ai, C. and E. C. Norton (2003). Interaction terms in logit and probit models. *Economics Letters 80*(1), 123–129.

Alary, D., C. Gollier, and N. Treich (2013). The effect of ambiguity aversion on insurance and self-protection. *The Economic Journal 123*(573), 1188–1202.

Allais, M. (1953). Le comportement de l'homme rationnel devant le risque: Critique des postulats et axiomes de l'école américaine. *Econometrica 21*(4), 503–546.

Andreoni, J. and C. Sprenger (2010). Certain and uncertain utility: The Allais paradox and five decision theory phenomena. Levine's Working Paper Archive.

Asch, S. E. (1956). Studies of independence and conformity: I. a minority of one against a unanimous majority. *Psychological Monographs: General and Applied 70*(9), 1–70.

Attanasio, O., A. Barr, J. C. Cardenas, G. Genicot, and C. Meghir (2012). Risk pooling, risk preferences, and social networks. *American Economic Journal: Applied Economics 4*(2), 134–167.

Bacharach, M. (1999). Interactive team reasoning: A contribution to the theory of cooperation. *Research in Economics 53*(2), 117–147.

Bacharach, M. (2006). *Beyond individual choice: Teams and frames in game theory.* Princeton: Princeton University Press.

Baillon, A. and H. Bleichrodt (2013). Testing ambiguity models through the measurement of probabilities for gains and losses. Working Paper.

Banerjee, A., A. G. Chandrasekhar, E. Duflo, and M. O. Jackson (2013). The diffusion of microfinance. *Science 341*(6144).

Bardsley, N., J. Mehta, C. Starmer, and R. Sugden (2010). Explaining focal points: Cognitive hierarchy theory versus team reasoning. *The Economic Journal 120*(543), 40–79.

Barr, A., M. Dekker, and M. Fafchamps (2012a). Bridging the gender divide: An experimental analysis of group formation in African villages. *World Development 40*(10), 2063 – 2077.

Barr, A., M. Dekker, and M. Fafchamps (2012b). Who shares risk with whom under different enforcement mechanisms? *Economic Development and Cultural Change 60*(4), 677–706.

Barr, A. and G. Genicot (2008). Risk sharing, commitment, and information: An experimental analysis. *Journal of the European Economic Association 6*(6), 1151–1185.

Bauer, M., A. Cassar, J. Chytilová, and J. Henrich (2014). War's enduring effects on the development of egalitarian motivations and in-group biases. *Psychological Science 25*(1), 47–57.

Bault, N., G. Coricelli, and A. Rustichini (2008). Interdependent utilities: How social ranking affects choice behavior. *PLoS ONE 3*(10), e3477.

Bazzi, S. and C. Blattman (2014). Economic shocks and conflict: The evidence from commodity prices. *American Economic Journal: Macroeconomics 4*(6), 1–38.

Berger, L., H. Bleichrodt, and L. Eeckhoudt (2013). Treatment decisions under ambiguity. *Journal of Health Economics 32*(3), 559–569.

Bernheim, B. D. (1994). A theory of conformity. *Journal of Political Economy 102*(5), 841–877.

Besley, T. J. and T. Persson (2008). The incidence of civil war: Theory and evidence. NBER Working Paper 14585.

Bikhchandani, S., D. Hirshleifer, and I. Welch (1998). Learning from the behavior of others: Conformity, fads, and informational cascades. *The Journal of Economic Perspectives 12*(3), 151–170.

Binmore, K., L. Stewart, and A. Voorhoeve (2012). How much ambiguity aversion? Finding indifferences between Ellsberg's risky and ambiguous bets. *Journal of Risk and Uncertainty 45*(3), 215–238.

Binswanger, H. P. (1980). Attitudes toward risk: Experimental measurement in rural India. *American Journal of Agricultural Economics 62*(3), 395–407.

Birnbaum, M. H. (1992). Violations of monotonicity and contextual effects in choice-based certainty equivalents. *Psychological Science 3*(5), 310–314.

Birnbaum, M. H., G. Coffey, B. A. Mellers, and R. Weiss (1992). Utility measurement: Configural-weight theory and the judge's point of view. *Journal of Experimental Psychology: Human Perception and Performance 18*(2), 331–346.

Birnbaum, M. H. and S. E. Sutton (1992). Scale convergence and utility measurement. *Organizational Behavior and Human Decision Processes 52*(2), 183–215.

Blattman, C. and E. Miguel (2010). Civil war. *Journal of Economic Literature 48*(1), 3–57.

Blount, S. (1995). When social outcomes aren't fair: The role of causal attributions on preferences. *Organizational Behavior and Human Decision Processes 63*, 131–44.

Bolton, G. E., J. Brandts, and A. Ockenfels (2005). Fair procedures: Evidence from games involving lotteries. *The Economic Journal 115*(506), 1054–1076.

Bolton, G. E. and A. Ockenfels (2000). ERC: A theory of equity, reciprocity and competition. *The American Economic Review 90*(1), 166–193.

Bossaerts, P., P. Ghirardato, G. Serena, and W. Zamel (2010). Prices and allocations in asset markets with heterogeneous attitudes toward ambiguity. *Review of Financial Studies 23*(4), 1325–1359.

Brañas-Garza, P., R. Cobo-Reyes, M. P. Espinosa, N. Jiménez, J. Kovářík, and G. Ponti (2010). Altruism and social integration. *Games and Economic Behavior 69*(2), 249–257.

Brandts, J. and G. Charness (2000). Hot vs. cold: Sequential responses and preference stability in experimental games. *Experimental Economics 2*, 227–238.

Broyden, C. G. (1970). The convergence of a class of double-rank minimization algorithms. *IMA Journal of Applied Mathematics 6*(1), 76–90.

Budescu, D. V., K. M. Kuhn, K. M. Kramer, and T. R. Johnson (2002). Modeling certainty equivalents for imprecise gambles. *Organizational Behavior and Human Decision Processes 88*(2), 748 – 768.

Burnstein, E. and A. Vinokur (1977). Persuasive argumentation and social comparison as determinants of attitude polarization. *Journal of Experimental Social Psychology 13*(4), 315–332.

Bursztyn, L., F. Ederer, B. Ferman, and N. Yuchtman (2014). Understanding mechanisms underlying peer effects: Evidence from a field experiment on financial decisions. *Econometrica 82*(4), 1273–1301.

Cai, J., A. De Janvry, and E. Sadoulet (forthcoming). Social networks and the decision to insure. *American Economic Journal: Applied Economics*.

Callen, M., M. Isaqzadeh, J. D. Long, and C. Sprenger (2014). Violence and risk preference: Experimental evidence from Afghanistan. *The American Economic Review 104*(1), 123–148.

Camerer, C. F. (2003). *Behavioral game theory: Experiments in strategic interaction*. Princeton: Princeton University Press.

Camerer, C. F., T.-H. Ho, and J.-K. Chong (2004). A cognitive hierarchy model of games. *The Quarterly Journal of Economics 119*(3), 861–898.

Camerer, C. F. and M. Weber (1992). Recent developments in modeling preferences: Uncertainty and ambiguity. *Journal of Risk and Uncertainty 5*(4), 325–370.

Cameron, A. C., J. B. Gelbach, and D. Miller (2011). Robust inference with multi-way clustering. *Journal of Business & Economic Statistics 29*(2), 238–249.

Cappelen, A. W., J. Konow, E. O. Sorensen, and B. Tungodden (2013). Just luck: An experimental study of risk taking and fairness. *The American Economic Review 103*(4), 1298–1413.

Card, D., A. Mas, E. Moretti, and E. Saez (2010). Inequality at work: The effect of peer salaries on job satisfaction. NBER Working Paper 16396.

Casey, J. T. and J. T. Scholz (1991). Boundary effects of vague risk information on taxpayer decisions. *Organizational Behavior and Human Decision Processes 50*(2), 360–394.

Cason, T. N. and V.-L. Mui (1998). Social influence in the sequential dictator game. *Journal of Mathematical Psychology 42*(2), 248–265.

Çelen, B. and S. Kariv (2004). Observational learning under imperfect information. *Games and Economic Behavior 47*(1), 72–86.

Chakravarty, S. and J. Roy (2009). Recursive expected utility and the separation of attitudes towards risk and ambiguity: An experimental study. *Theory and Decision 66*(3), 199–228.

Charness, G., E. Karni, and D. Levin (2007). Individual and group decision making under risk: An experimental study of bayesian updating and violations of first-order stochastic dominance. *Journal of Risk and Uncertainty 35*(2), 129–148.

Charness, G., E. Karni, and D. Levin (2013). Ambiguity attitudes and social interactions: An experimental investigation. *Journal of Risk and Uncertainty 46*(1), 1–25.

Cialdini, R. B. and N. J. Goldstein (2004). Social influence: Compliance and conformity. *Annual Review of Psychology 55*(1), 591–621.

Cialdini, R. B. and M. R. Trost (1998). Social influence: Social norms, conformity and compliance. *The Handbook of Social Psychology 55*(2), 151–192.

Clark, A. E. and A. J. Oswald (1998). Comparison-concave utility and following behavior in social and economic settings. *Journal of Public Economics 70*, 133–155.

Cohen, M., J.-Y. Jaffray, and T. Said (1987). Experimental comparison of individual behavior under risk and under uncertainty for gains and for losses. *Organizational Behavior and Human Decision Processes 39*(1), 1–22.

Collard, F., S. Mukerji, K. Sheppard, and J.-M. Tallon (2011). Ambiguity and the historical equity premium. CES Working Paper No. 2011.32.

Colman, A. M., B. D. Pulford, and J. Rose (2008). Collective rationality in interactive decisions: Evidence for team reasoning. *Acta Psychologica 128*(2), 387–397.

Conradt, L. and C. List (2010). Group decisions in humans and animals: A survey. *Philosophical Transactions of the Royal Society B 364*(1518), 719–742.

Conte, A., J. D. Hey, and P. G. Moffatt (2011). Mixture models of choice under risk. *Journal of Econometrics 162*(1), 79–88.

Cooper, D. J. and M. Rege (2011). Misery loves company: Social regret and social interaction effects in choices under risk and uncertainty. *Games and Economic Behavior 73*(1), 91–110.

Coser, L. A. (1956). *The functions of social conflict.* Abingdon: Routledge.

Cubitt, R. P. and R. Sugden (2003). Common knowledge, salience and convention: A reconstruction of David Lewis' game theory. *Economics and Philosophy 19*(2), 175–210.

Curley, S. P. and J. F. Yates (1989). An empirical evaluation of descriptive models of ambiguity reactions in choice situations. *Journal of Mathematical Psychology 33*(4), 397–427.

Curley, S. P., J. F. Yates, and R. A. Abrams (1986). Psychological sources of ambiguity avoidance. *Organizational Behavior and Human Decision Processes 38*(2), 230–256.

De Lara Resende, J. G. and G. Wu (2010). Competence effects for choices involving gains and losses. *Journal of Risk and Uncertainty 40*(2), 109–132.

De Weerdt, J. and S. Dercon (2006). Risk-sharing networks and insurance against illness. *Journal of Development Economics 81*(2), 337 – 356.

Dercon, S. and P. Krishnan (2000). In sickness and in health: Risk sharing within households in rural Ethiopia. *Journal of Political Economy 108*(4), 688–727.

Deutsch, M. (1969). Conflicts: Productive and destructive. *Journal of Social Issues 25*(1), 7–42.

Di Mauro, C. and A. Maffioletti (1996). An experimental investigation of the impact of ambiguity on the valuationof self-insurance and self-protection. *Journal of Risk and Uncertainty 13*(1), 53–71.

Di Mauro, C. and A. Maffioletti (2004). Attitudes to risk and attitudes to uncertainty: Experimental evidence. *Applied Economics 36*(4), 357–372.

Dimmock, S. G., R. Kouwenberg, O. Mitchell, and K. Peijnenburg (2013). Ambiguity aversion and household portfolio choice: Empirical evidence. NBER Working Paper 18743.

Dimmock, S. G., R. Kouwenberg, and P. P. Wakker (2012). Ambiguity attitudes in a large representative sample. Working Paper.

Dohmen, T., A. Falk, D. Huffman, and U. Sunde (2012). The intergenerational transmission of risk and trust attitudes. *The Review of Economic Studies 79*(2), 645–677.

Dohmen, T., A. Falk, D. Huffman, U. Sunde, J. Schupp, and G. G. Wagner (2011). Individual risk attitudes: Measurement, determinants, and behavioral consequences. *Journal of the European Economic Association 9*(3), 522–550.

Dow, J. and S. R. d. C. Werlang (1992). Uncertainty aversion, risk aversion, and the optimal choice of portfolio. *Econometrica 60*(1), 197–204.

Du, N. and D. V. Budescu (2005). The effects of imprecise probabilities and outcomes in evaluating investment options. *Management Science 51*(12), 1791–1803.

Dube, O. and J. F. Vargas (2013). Commodity price shocks and civil conflict: Evidence from Colombia. *The Review of Economic Studies 80*(4), 1384–1421.

Duesenberry, J. S. (1949). *Income, saving and the theory of consumer behavior.* Cambridge: Harvard University Press.

Duflo, E., P. Dupas, and M. Kremer (2011). Peer effects, teacher incentives, and the impact of tracking: Evidence from a randomized evaluation in Kenya. *The American Economic Review 101*(5), 1739–1774.

Duflo, E. and E. Saez (2002). Participation and investment decisions in a retirement plan: The influence of colleagues' choices. *Journal of Public Economics 85*, 121–148.

Dürsch, P., D. Römer, and B. Roth (2013). Intertemporal stability of ambiguity preferences. Working Paper, University of Heidelberg.

Easley, D. and M. O'Hara (2009). Ambiguity and nonparticipation: The role of regulation. *Review of Financial Studies 22*(5), 1817–1843.

Einhorn, H. J. and R. M. Hogarth (1986). Decision making under ambiguity. *Journal of Business 59*(2), 225–250.

Eisenberger, R. and M. Weber (1995). Willingness-to-pay and willingness-to-accept for risky and ambiguous lotteries. *Journal of Risk and Uncertainty 10*(3), 223–233.

Ellsberg, D. (1961). Risk, ambiguity, and the savage axioms. *The Quarterly Journal of Economics 75*(4), 643–669.

Ellsberg, D. (1963). [Risk, ambiguity, and the savage axioms]: Reply. *The Quarterly Journal of Economics 77*(2), 336–342.

Ellsberg, D. (2011). Notes on the origin of the Ellsberg urns (introduction to the symposium issue). *Economic Theory 48*(2-3), 219–227.

Epstein, L. G. and J. Miao (2003). A two-person dynamic equilibrium under ambiguity. *Journal of Economic Dynamics and Control 27*(7), 1253–1288.

Epstein, L. G. and M. Schneider (2003). Recursive multiple-priors. *Journal of Economic Theory 113*(1), 1–31.

Epstein, L. G. and M. Schneider (2007). Learning under ambiguity. *The Review of Economic Studies 74*(4), 1275–1303.

Epstein, L. G. and T. Wang (1994). Intertemporal asset pricing under knightian uncertainty. *Econometrica 62*(2), 283–322.

Esteban, J.-M. and D. Ray (1994). On the measurement of polarization. *Econometrica 62*(4), 819–851.

Esteban, J.-M. and D. Ray (2011). Linking conflict to inequality and polarization. *The American Economic Review 101*(4), 1345–1374.

Fafchamps, M. (2003). *Rural poverty, risk and development.* Cheltenham: Edward Elgar Publishing.

Fafchamps, M. and F. Gubert (2007). The formation of risk sharing networks. *Journal of Development Economics 83*(2), 326–350.

Fafchamps, M. and S. Lund (2003). Risk-sharing networks in rural Philippines. *Journal of Development Economics 71*(2), 261–287.

Falk, A. and U. Fischbacher (2002). "Crime" in the lab-detecting social interaction. *European Economic Review 46*(4-5), 859–869.

Falk, A. and U. Fischbacher (2006). A theory of reciprocity. *Games and Economic Behavior 54*(2), 293–315.

Falk, A., U. Fischbacher, and S. Gächter (2013). Living in two neighborhoods – Social interaction effects in the laboratory. *Economic Inquiry 51*(1), 563–578.

Falk, A. and A. Ichino (2006). Clean evidence on peer effects. *Journal of Labor Economics 24*(1), 39–57.

Farber, D. A. (2011). Uncertainty. *Georgetown Law Journal 99*, 901–959.

Fearon, J. D. (1995). Rationalist explanations for war. *International Organization 49*, 379–414.

Fehr, E. and K. M. Schmidt (1999). A theory of fairness, competition, and cooperation. *The Quarterly Journal of Economics 114*(3), 817–868.

Fehr, E. and K. M. Schmidt (2006). Chapter 8: The economics of fairness, reciprocity and altruism – experimental evidence and new theories. In S.-C. Kolm and J. M. Ythier (Eds.), *Foundations*, Volume 1 of *Handbook of the Economics of Giving, Altruism and Reciprocity.* Elsevier.

Fellner, W. (1961). Two propositions in the theory of induced innovations. *The Economic Journal 71*(282), 305–308.

Festinger, L. (1950). Informal social communication. *Psychological Review 57*, 271–282.

Festinger, L. (1954). A theory of social comparison processes. *Human Relations 7*(2), 117–140.

Filmer, D. and L. H. Pritchett (2001). Estimating wealth effects without expenditure data – Or tears: An application to educational enrolments in states of India. *Demography 38*(1), 115–132.

Fischbacher, U. (2007). z-tree: Zurich toolbox for ready-made economic experiments. *Experimental Economics 20*(2), 171–178.

Fletcher, R. (1970). A new approach to variable metric algorithms. *The Computer Journal 13*(3), 317–322.

Fox, C. R. and A. Tversky (1998). A belief-based account of decision under uncertainty. *Management Science 44*(7), 879–895.

Fox, C. R. and M. Weber (2002). Ambiguity aversion, comparative ignorance, and decision context. *Organizational Behavior and Human Decision Processes 88*(1), 476–498.

Friedl, A., K. Lima de Miranda, and U. Schmidt (2014). Insurance demand and social comparison: An experimental analysis. *Journal of Risk and Uncertainty 48*(2), 97–109.

Gächter, S., D. Nosenzo, and M. Sefton (2013). Peer effects in pro-social behavior: Social norms or social preferences? *Journal of the European Economic Association 11*(3), 548–573.

Galí, J. (1994). Keeping up with the Joneses: Consumption externalities, portfolio choice, and asset prices. *Journal of Money, Credit and Banking 26*(1), 1–8.

Gärdenfors, P. (1979). Forecasts, decisions and uncertain probabilities. *Erkenntnis 14*(2), 159–181.

Gebhardt, G. (2004). Inequity aversion, financial markets, and output fluctuations. *Journal of the European Economic Association 2*(2-3), 229–239.

Gebhardt, G. (2011). Investment decisions with loss aversion over relative consumption. *Journal of Economic Behavior and Organization 80*(1), 68–73.

Georgarakos, D., M. Haliassos, and G. Pasini (2013). Household debt and social interactions. SSRN Working Paper No. 2014222.

Gilboa, I. and D. Schmeidler (1989). Maxmin expected utility with non-unique prior. *Journal of Mathematical Economics 18*(2), 141 – 153.

Gintis, H. (2009). *The bounds of reason: Game theory and the unification of the behavioral sciences*. Princeton: Princeton University Press.

Gintis, H. (2010). Social norms as choreography. *politics, philosophy & economics 9*(3), 251–264.

Gneezy, U., K. L. Leonard, and J. A. List (2009). Gender differences in competition: Evidence from a matrilineal and a patriarchal society. *Econometrica 77*(5), 1637–1664.

Gneezy, U. and J. Potters (1997). An experiment on risk taking and evaluation periods. *The Quarterly Journal of Economics 112*(2), 63–645.

Goeree, J. K., M. A. McConnell, T. Mitchell, T. Tromp, and L. Yariv (2010). The 1/d law of giving. *American Economic Journal: Microeconomics 2*(1), 183–203.

Goeree, J. K. and L. Yariv (2007). Conformity in the lab. Working Paper.

Gold, N. and R. Sugden (2007). Collective intentions and team agency. *The Journal of Philosophy 104*(3), 109–137.

Gollier, C. (2011). Portfolio choices and asset prices: The comparative statics of ambiguity aversion. *The Review of Economic Studies 78*(4), 1329–1344.

Harrison, G. W. and E. Rutström (2009). Expected utility theory and prospect theory: one wedding and a decent funeral. *Experimental Economics 12*, 133–158.

Heald, S. (1998). *Controlling anger: The anthropology of Gisu violence*. Eastern African studies. Athens: Ohio University Press.

Heath, C. and A. Tversky (1991). Preference and belief: Ambiguity and competence in choice under uncertainty. *Journal of Risk and Uncertainty 4*(1), 5–28.

Henrich, J. and R. McElreath (2002). Are peasants risk–averse decision makers? *Current Anthropology 43*(1), 172–181.

Hey, J. D. and C. Orme (1994). Investigating generalizations of expected utility theory using experimental data. *Econometrica 62*(6), 1291–1326.

Hindriks, F. (2012). Team reasoning and group identification. *Rationality and Society 24*(2), 198–220.

Ho, J. L., L. R. Keller, and P. Keltyka (2002). Effects of outcome and probabilistic ambiguity on managerial choices. *Journal of Risk and Uncertainty 24*(1), 47–74.

Hodgson, D. H. (1967). *Consequences of Utilitarianism: A study in normative ethics and legal theory.* Oxford: Clarendon.

Hogarth, R. M. and H. Kunreuther (1985). Ambiguity and insurance decisions. *The American Economic Review 75*(2), 386–390.

Hogarth, R. M. and H. Kunreuther (1989). Risk, ambiguity, and insurance. *Journal of Risk and Uncertainty 2*(1), 5–35.

Holt, C. A. and S. K. Laury (2002). Risk aversion and incentive effects. *The American Economic Review 92*(5), 1644–1655.

Hong, H., J. D. Kubik, and J. C. Stein (2004). Social interaction and stock-market participation. *The Journal of Finance 59*(1), 137–163.

Hopkins, E. and T. Kornienko (2004). Running to keep in the same place: Consumer choice as a game of status. *The American Economic Review 94*(4), 1085–1107.

Hoy, M., R. Peter, and A. Richter (2014). Take-up for genetic tests and ambiguity. *Journal of Risk and Uncertainty 48*(2), 111–133.

Huber, P. J. (1973). The use of choquet capacities in statistics. *Bulletin of the International Statistical Institute 45*, 181–188.

Isenberg, D. J. (1986). Group polarization: A critical review and meta-analysis. *Journal of Personality and Social Psychology 50*, 1141–1151.

Ju, N. and J. Miao (2012). Ambiguity, learning, and asset returns. *Econometrica 80*(2), 559–591.

Kahn, B. E. and R. K. Sarin (1988). Modeling ambiguity in decisions under uncertainty. *Journal of Consumer Research 15*(2), 265–272.

Kahneman, D. (1992). Reference points, anchors, norms, and mixed feelings. *Organizational behavior and human decision processes 51*(2), 296–312.

Kahneman, D. and A. Tversky (1979). Prospect theory: An analysis of decision under risk. *Econometrica 47*(2), 263–292.

Kahneman, D. and A. Tversky (1984). Choices, values, and frames. *American Psychologist 39*(4), 341–350.

Kahneman, D. and A. Tversky (1986). Rational choice and the framing of decisions. *The Journal of Business 59*(4), 251–278.

Karamagi, C., J. Tumwine, T. Tylleskar, and K. Heggenhougen (2006). Intimate partner violence against women in eastern Uganda: Implications for HIV prevention. *BMC Public Health 6*(1), 1–12.

Karlan, D., M. Mobius, T. Rosenblat, and A. Szeidl (2009). Trust and social collateral. *The Quarterly Journal of Economics 124*(3), 1307–1361.

Kast, F., S. Meier, and D. Pomeranz (2012). Under-savers anonymous: Evidence on self-help groups and peer pressure as a savings commitment device. *IZA Discussion Paper* (6311).

Keck, S., E. Diecidue, and D. Budescu (2011). Group decision making under ambiguity. Working Paper.

Keller, L. R., R. K. Sarin, and J. Sounderpandian (2007). An examination of ambiguity aversion: Are two heads better than one? *Judgment and Decision Making 2*(6), 390–397.

Kennan, J. and R. Wilson (1993). Bargaining with private information. *Journal of Economic Literature 31*(1), 45–104.

Keren, G. and L. E. Gerritsen (1999). On the robustness and possible accounts of ambiguity aversion. *Acta Psychologica 103*(1), 149–172.

Keynes, J. M. (1921). *A treatise on probability.* London: Macmillan & Co.

Klibanoff, P., M. Marinacci, and S. Mukerji (2005). A smooth model of decision making under ambiguity. *Econometrica 73*(6), 1849–1892.

Kliemt, H. (2009). *Philosophy and economics I: Methods and models.* Munich: Oldenbourg.

Knight, F. H. (1921). *Risk, uncertainty and profit.* New York: Hart, Schaffner and Marx.

Kothiyal, A., V. Spinu, and P. Wakker (2014). An experimental test of prospect theory for predicting choice under ambiguity. *Journal of Risk and Uncertainty 48*(1), 1–17.

La Fontaine, J. S. (1959). *The Gisu of Uganda.* London: International African Institute.

Lahno, B. (2007). Rational choice and rule-following behavior. *Rationality and Society 19*(4), 425–450.

Lahno, B. (2010). Norms as reasons for action. *Archiv für Rechts-und Sozialphilosphie 95*(4), 563–578.

Leider, S., M. M. Möbius, T. Rosenblat, and Q.-A. Do (2009). Directed altruism and enforced reciprocity in social networks. *The Quarterly Journal of Economics 124*(4), 1815–1851.

Levy, I., J. Snell, A. J. Nelson, A. Rustichini, and P. W. Glimcher (2010). Neural representation of subjective value under risk and ambiguity. *Journal of Neurophysiology 103*(2), 1036–1047.

Linde, J. and J. Sonnemans (2012). Social comparison and risky choices. *Journal of Risk and Uncertainty 44*(1), 45–72.

Lipton, M. (1968). The theory of optimising peasant. *The Journal of development Studies 3*, 327–351.

Liu, E. M. (2013). Time to change what to sow: Risk preferences and technology adoption decisions of cotton farmers in China. *The Review of Economics and Statistics 95*(4), 1386–1403.

Loomes, G. (2005). Modelling the stochastic component of behaviour in experiments: Some issues for the interpretation of data. *Experimental Economics 8*, 301–323.

Maccheroni, F., M. Marinacci, and A. Rustichini (2012). Social decision theory: Choosing within and between groups. *The Review of Economic Studies 79*, 1591–1636.

Machina, M. J. and D. Schmeidler (1992). A more robust definition of subjective probability. *Econometrica 60*(4), 745–780.

Maenhout, P. J. (2004). Robust portfolio rules and asset pricing. *Review of Financial Studies 17*(4), 951–983.

Mangelsdorff, L. and M. Weber (1994). Testing choquet expected utility. *Journal of Economic Behavior & Organization 25*(3), 437 – 457.

Manski, C. F. (1993). Identification of endogenous social effects: The reflection problem. *The Review of Economic Studies 60*(3), 531–542.

Manski, C. F. (2000). Economic analysis of social interactions. *The Journal of Economic Perspectives 14*(3), 115–136.

Marinacci, M. (2002). Learning from ambiguous urns. *Statistical Papers 43*(1), 143–151.

Mas, A. and E. Moretti (2009). Peers at work. *The American Economic Review 99*(1), 112–145.

Mazzocco, M. and S. Saini (2012). Testing efficient risk sharing with heterogeneous risk preferences. *The American Economic Review 102*(1), 428–68.

Miguel, E., S. Satyanath, and E. Sergenti (2004). Economic shocks and civil conflict: An instrumental variables approach. *Journal of Political Economy 112*(4), 725–753.

Millner, A., S. Dietz, and G. Heal (2010). Ambiguity and climate policy. Centre for Climate Change Economics and Policy Working Paper No. 28.

Ministry of Water & Environment Uganda (2010). *Report of the Directorate of Water Development.* Ministry of Water & Environment Uganda.

Muthukrishnan, A. V., L. Wathieu, and A. J. Xu (2009). Ambiguity aversion and the preference for established brands. *Management Science 55*(12), 1933–1941.

Norman, D. W. (1974). Rationalising mixed cropping under indigenous conditions: The example of northern Nigeria. *Journal of Development Studies 11*(1), 3–21.

Noussair, C. N., S. T. Trautmann, G. Kuilen, and N. Vellekoop (2013). Risk aversion and religion. *Journal of Risk and Uncertainty 47*(2), 165–183.

Pelled, L. H., K. M. Eisenhardt, and K. R. Xin (1999). Exploring the black box: An analysis of work group diversity, conflict and performance. *Administrative Science Quarterly 44*(1), 1–28.

Petersen, M. A. (2009). Estimating standard errors in finance panel data sets: Comparing approaches. *Review of Financial Studies 22*(1), 435–480.

Quiggin, J. (1982). A theory of anticipated utility. *Journal of Economic Behavior & Organization 3*(4), 323–343.

Quiggin, J. (1993). *Generalized expected utility theory: The rank dependent model.* Dordrecht: Springer Science+Business Media.

Raven, J., J. C. Raven, and J. H. Court (1998). *Manual for Raven's progressive matrices and vocabulary scales.* Oxford Psychologists Press.

Regan, D. (1980). *Utilitarianism and cooperation.* Oxford University Press.

Renneboog, L. and C. Spaenjers (2012). Religion, economic attitudes, and household finance. *Oxford Economic Papers 64*(1), 103–127.

Roberts, H. V. (1963). Risk, ambiguity, and the savage axioms: Comment. *The Quarterly Journal of Economics 77*(2), 327–336.

Rohde, I. and K. Rohde (2011). Risk attitudes in a social context. *Journal of Risk and Uncertainty 43*(3), 205–225.

Rohner, D., M. Thoenig, and F. Zilibotti (2013). Seeds of distrust: Conflict in Uganda. *Journal of Economic Growth 18*(3), 217–252.

Roscoe, J. (1924). *The Bagesu and other tribes of the Uganda protectorate*. Cambridge: Cambridge University Press.

Roth, A. E., J. K. Murnighan, and F. Schoumaker (1988). The deadline effect in bargaining: Some experimental evidence. *The American Economic Review 78*(4), 806–823.

Sacerdote, B. (2001). Peer effects with random assignment: Results for dartmouth roommates. *Quarterly Journal of Economics 116*(2), 681–704.

Sacerdote, B. (2011). Peer effects in education: How might they work, how big are they and how much do we know thus far? In E. Hanushek, S. Machin, and L. Woessmann (Eds.), *Handbook of the Economics of Education*, Volume 3. Elsevier.

Saito, K. (2013). Social preferences under risk: Equality of opportunity versus equality of outcome. *The American Economic Review 103*(7), 3084–3101.

Savage, Leonard, J. (1954). *The foundations of statistics*. New York: Wiley.

Schluter, M. G. G. and T. D. Mount (1976). Some management objectives of the peasant farmer: An analysis of risk aversion in the choice of cropping pattern, Surat district, India. *Journal of Development Studies 12*(3), 246–261.

Scott, J. C. (1976). *The moral economy of the peasant: Rebellion and subsistence in Southeast Asia*. New Haven: Yale University Press.

Shiller, R. J. (1984). Stock prices and social dynamics. *Brookings Papers on Economic Activity 2*, 457–498.

Simmel, G. (1955). *Conflict and the web of group affiliations*. New York: Free Press. Translated by K. H. Wolff and R. Bendix.

Snow, A. (2011). Ambiguity aversion and the propensities for self-insurance and self-protection. *Journal of Risk and Uncertainty 42*(1), 27–43.

Sprenger, C. (2010). An endowment effect for risk: Experimental tests of stochastic reference points. Working Paper.

Stahl, D. O. (2014). Heterogeneity of ambiguity preferences. *Review of Economics and Statistics 96*(4), 609–617.

Sugden, R. (1991). Rational choice: a survey of contributions from economics and philosophy. *The Economic Journal 101*(407), 751–785.

Sugden, R. (1993). Thinking as a team: Towards an explanation of nonselfish behavior. *Social Philosophy and Policy 10*(1), 69–89.

Sugden, R. (2000). Team preferences. *Economics and Philosophy 16*(2), 175–204.

Sugden, R. (2003). The logic of team reasoning. *Philosophical Explorations 6*(3), 165–181.

Thompson, S. B. (2011). Simple formulas for standard errors that cluster by both firm and time. *Journal of Financial Economics 99*(1), 1–10.

Toda, M. and E. H. Shuford (1965). Utility, induced utilities, and small worlds. *Behavioral Science 10*(3), 238–254.

Townsend, R. M. (1994). Risk and insurance in village India. *Econometrica 62*(3), 539–591.

Trautmann, S. T. (2009). A tractable model of process fairness under risk. *Journal of Economic Psychology 30*(5), 803–813.

Trautmann, S. T. and G. van de Kuilen (forthcoming). Chapter 11: Ambiguity attitudes. In G. Keren and G. Wu (Eds.), *Blackwell Handbook of Judgment and Decision Making,*. Malden: Blackwell.

Trautmann, S. T. and F. M. Vieider (2011). Social influences on risk attitudes: Applications in economics. In S. Roeser (Ed.), *Handbook of Risk Theory*. Springer.

Trautmann, S. T., F. M. Vieider, and P. P. Wakker (2008). Causes of ambiguity aversion: Known versus unknown preferences. *Journal of Risk and Uncertainty 36*(3), 225–243.

Trautmann, S. T., F. M. Vieider, and P. P. Wakker (2011). Preference reversals for ambiguity aversion. *Management Science 57*(7), 1320–1333.

Trautmann, S. T. and P. P. Wakker (2012). Making the Anscombe-Aumann approach to ambiguity suited for descriptive applications. Working Paper, Erasmus University.

Tversky, A. and D. Kahneman (1991). Loss aversion in riskless choice: A reference-dependent model. *The Quarterly Journal of Economics 106*(4), 1039–1061.

Tversky, A. and D. Kahneman (1992). Advances in prospect theory: Cumulative representation of uncertainty. *Journal of Risk and Uncertainty 5*(4), 297–323.

Tversky, A., P. Slovic, and D. Kahneman (1990). The causes of preference reversal. *The American Economic Review 80*(1), 204–217.

Tymula, A., P. W. Glimcher, I. Levy, and L. A. Rosenberg Belmaker (2012). Separating risk and ambiguity preferences across the life span: Novel findings and implications for policy. Working Paper, New York University.

Veblen, T. B. (1899). *The theory of the leisure class: An economic study in the evolution of institutions*. New York: Macmillan.

Vieider, F., P. Martinsson, and M. Haileselassie (2012). Stake effects on ambiguity attitudes for gains and losses. Working Paper, WZB Berlin.

Viscusi, W. and H. Chesson (1999). Hopes and fears: the conflicting effects of risk ambiguity. *Theory and Decision 47*(2), 157–184.

Viscusi, W. K., O. R. Phillips, and S. Kroll (2011). Risky investments decisions: How are individuals influenced by their groups? *Journal of Risk and Uncertainty 43*(2), 81–106.

von Gaudecker, H.-M., A. van Soest, and E. Wengström (2011). Heterogeneity in risky choice behavior in a broad population. *The American Economic Review 101*(2), 664–694.

von Neumann, J. and O. Morgenstern (1947). *The theory of games and economic behavior*. Princeton: Princeton University Press.

Voors, M. J., E. E. Nillesen, P. Verwimp, E. H. Bulte, R. Lensink, and D. P. Van Soest (2012). Violent conflict and behavior: A field experiment in Burundi. *The American Economic Review 102*(2), 941–964.

Vuong, Q. H. (1989). Likelihood ratio tests for model selection and non-nested hypotheses. *Econometrica 57*(2), pp. 307–333.

Walley, P. (1991). *Statistical reasoning with imprecise probabilities*. London: Chapman and Hall.

Watson, D. and R. Friend (1969). Measurement of social-evaluative anxiety. *Journal of Consulting and Clinical Psychology 33*(4), 448–457.

Wolgin, J. M. (1975). Resource allocation and risk: A case study of smallholder agriculture in Kenya. *American Journal of Agricultural Economics 57*(4), 622–630.

Eidesstattliche Versicherung

Ich versichere hiermit eidesstattlich, dass ich die vorliegende Arbeit selbstständig und ohne fremde Hilfe verfasst habe. Die aus fremden Quellen direkt oder indirekt übernommenen Gedanken sowie mir gegebene Anregungen sind als solche kenntlich gemacht.
Die Arbeit wurde bisher keiner anderen Prüfungsbehörde vorgelegt und auch noch nicht veröffentlicht. Sofern ein Teil der Arbeit aus bereits veröffentlichten Papers besteht, habe ich dies ausdrücklich angegeben.

23. Juni 2014

Amrei M. Lahno